COLD INJURY

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THE JOSIAH MACY, JR FOUNDATION CONFERENCE PROGRAM

During the last fifteen years the Josiah Macy, Jr Foundation has organized more than twenty conference groups each group meeting for at least two days annually over a period of five or more years Each meeting is limited to twenty five participants (members and guests) selected to represent a multidiscipline approach to some urgent problem in the field of medicine and health The goal of this conference program is the promotion of communication the exchange of ideas and the stimulation of creativity among the participants The purpose of the publication of the Transactions of the meetings is to share as far as possible the conference process with a larger audience than could participate personally in the discussions

These conferences provide an opportunity for informal give and take among the participants To further this purpose the number of presentations planned for each day is generally restricted to one or two The member or guest selected to give such a presentation is requested not to read a paper but rather to highlight in an informal manner some of the more interesting aspects of his or her research with the expectation that there will be frequent interruptions by participants in the form of questions criticism or comment Such interruptions during the course of a presentation are encouraged and form an essential part of the group interchange

The conference program has always been viewed by the Foundation as an experiment in communication in which there is room for improvement and need for frequent reappraisal Sufficient experience has already been gained to justify the conclusion that this type of conference is an effective way of improving understanding among scientists in medicine and allied disciplines of broadening perspectives of changing attitudes and of overcoming prejudices The further conclusion has been reached as the result of this experiment that the major obstructions to understanding among scientists lie in the resistance of human attitudes to change rather than in difficulties of technical comprehension Less extensive experience with non scientists has indicated that the effectiveness of this type

of conference is not limited to groups of scientists but will function in any group meeting where most effective communication is the primary goal. It is also clear that the same conference technique with minor changes is readily adapted to small international conferences.

The style of publication of the Transactions has aroused considerable interest and some criticism. The criticism has been directed primarily to editorial permissiveness which has allowed in the final text in some instances too many questions remarks or comments which although perhaps useful during a heated discussion seem out of context and interrupt the sequence of thought in the printed volume. A few have objected to the principle of publishing in this style and would prefer a depersonalized summary without interruptions.

The Foundation staff and the Scientific Editors of these volumes welcome criticism and hope to profit thereby in increasing the usefulness of the Transactions to scientists and students of science in this country and abroad.

FRANK FREMONT-SMITH, M.D.
Medical Director

REDUCING A LARGE EFFICIENT HOMOTHERM TO A POIKILOTHERMIC STATUS

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IN MY PRESENTATION I WILL DISCUSS one test situation and six experimental variables Figure 1 illustrates the test situation, however, Figures 1 to 20 (with the exception of Figures 4, 8, 10 11, and 19)

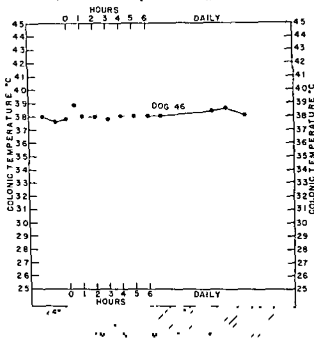


FIGURE 1 Core temperature (deep colonic) response to an abrupt moderately heavy cooling load (air temperature 3°C) in an unoperated healthy mongrel dog (dog 46) There was an immediate rise of core temperature from 38° to 39°C after which it returned to and was maintained at 38°C for the duration of the exposure
= shivering

will be the same, as far as ordinates and abscissas are concerned The end point recorded in Figure 1 is the deep colonic temperature of a mongrel dog, after it had been subjected abruptly to a heavy cooling load, as indicated at the bottom of the figure

The animal was taken from its cage located in a neutral environment (24°C) and placed in a similar cage located in a refrigerated room in this case 3°C Then it was equally abruptly removed from the cold and returned to its original cage During the animal's sojourn in the cold, it did not have benefit of food in its gastrointestinal tract nor was it benefited by previous conditioning to cold

The end point followed in all instances was core temperature In this case, it was deep colonic temperature, which is reasonably identical with the esophageal temperature, or any other temperature in the core area one might wish to observe

As soon as the animal was placed in the cold colonic temperature rose to reach 39°C in 30 minutes in this particular experiment The animal began to shiver, as indicated by the broken line between the points (in all the figures that I will show, a broken line indicates shivering), but core temperature rapidly returned to the *homothermic* level (38°C) where it remained during the 6 hour exposure

Fremont Smith Is that initial rise very common?

Keller Yes However, there is considerable minor variation in response from one animal to another, and from the same animal on different exposures The important point is that hypothermia is not allowed to develop, which Figure 2 shows Placing this same dog in a colder environment (−20°C) occasioned a rise in core temperature to 39° C where it was maintained for the duration of a 24 hour exposure When the two responses shown in Figures 1 and 2 are superimposed (Figure 3) (1), it is quite evident that the heavier the cooling load the more vigorous is the coordinated physiological resistance to hypothermia

Fremont Smith He overshoots the mark and really gets an elevated temperature

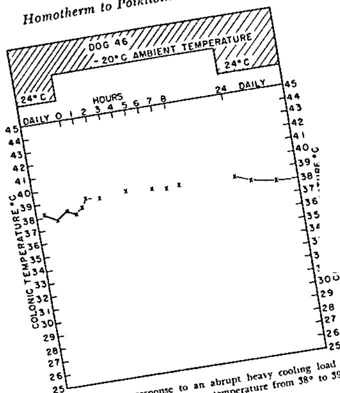
Keller That is right However, it is not fever, it is physiology

Fremont Smith The animal uses some of the same mechanisms to get his fever He shivers and he cuts down vasodilation

Keller Yes but it is not the usual integration

Fremont Smith No

Keller There isn t any question about the end result of no core hypothermia in the healthy dog under cooling loads which are



IGURE 2 Core temperature response to an abrupt heavy cooling load (—20 C) in Dog 46 There was an immediate rise of core temperature from 38° to 39°C where it was maintained for the duration of the exposure
= shivering
—— = absence of shivering as determined by gross clinical inspection

within physiological tolerances The fact that so many physiological processes are activated to attain this algebraic end result makes it difficult to isolate and investigate a single process Sometimes it is difficult to determine whether a process that is under suspicion is actually activated because of the many processes acting simultaneously

The experiments to be described were designed to attempt a physiological dissociation of the mechanisms utilized to combat cold in the homotherm The dog is considered to be an efficient homotherm and we use the word efficient with reference to this animal s over all ability to prevent a lowering of core temperature and not necessarily with reference to its efficiency in converting body tissues (fuel) to heat

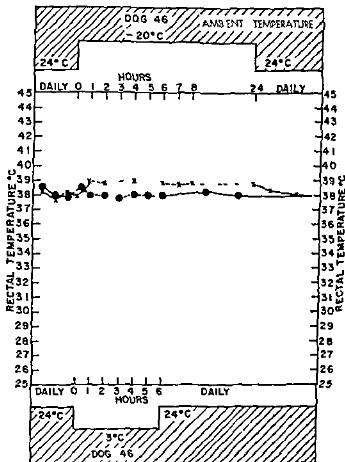


FIGURE 3 Superimposed core temperature curves for dog 46 shown in Figures 1 and 2 It is read ly evident that the heavier the cool ng load the more vigorous is the physiological resistance to a core hypothermia

I think the subsequent presentation can best be centered around the projected question to what extent can the homotherm s ability to resist hypothermia be gradedly and permanently impaired by selective surgical ablations? The reason for the words gradedly and permanently will become obvious as the experiments are presented

The first surgical variable is that of total removal of the vertebral sympathetic chains (Figure 4) The removal of the sympathetic chains in the dog is somewhat difficult (as compared to the human for instance) because they are so small and fragile, particularly the

connecting trunks between the ganglia Therefore, our criteria for *complete removal are unbroken surgical specimens*

In the case of dog 144, as is shown by the surgical specimen in Figure 4, we succeeded in meeting these criteria except perhaps on the right side, up just below the stellate ganglion, where it appears there is a partial break Yet a small strand is visible In such a case, one ganglion might have stayed in place It is extremely difficult to make sure, even by careful autopsy, whether or not all the ganglia have been removed unless the unbroken chain is in a specimen jar

The primary reason for executing this surgical variable was to remove the centrally activated vasoconstrictor nerve impulses which travel by way of these chains to reach the blood vessels, so as to determine whether or not the total absence of these impulses produces a deficit in the animal's over all resistance to hypothermia

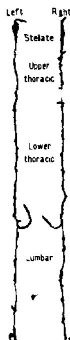


FIGURE 4 The vertebral sympathetic chains removed from dog 144 The criterion for complete removal is the unbroken chain as a surgical specimen

The statement of the problem is not uncomplicated because this surgical procedure also removes the adrenergic nerve supply to the liver and the cholinergic innervation of the adrenal medulla For some time the literature has implied that both of these organs are associated with cold stimulated thermogenesis

When the animal shown in Figure 5 was placed in the 3°C ambient temperature, for 3 hours it maintained a core temperature at the 38°C level These animals do not become hypothermic directly following operation when housed in a neutral external environment, they actually have a slightly elevated core temperature under these conditions Shivering is much more vigorous than in the unoperated animal, as first pointed out by Cannon (2) when he did this operation in the cat

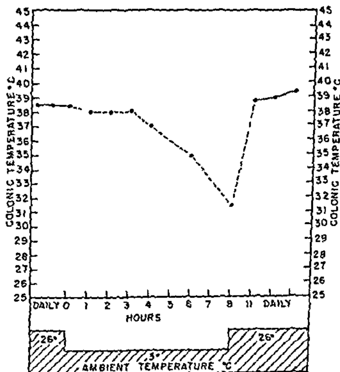


FIGURE 5 Core temperature response in dog 144 during the second week after total removal of the vertebral sympathetic chains (Figure 4) when abruptly *subjected to a moderate* cooling load (3°C) A slight hyperthermia was evident when the dog was housed in a neutral environment A core temperature in the normal range was maintained for 3 hours after the dog had been subjected to the cooling load *Hypothermia* rapidly developed subsequent to 3 hours of exposure

Fremont Smith The animal did not overshoot the mark in this case There is no rise in temperature such as most of the others showed

Keller That is right The core temperature came back down from a slightly hyperthermic level and vigorous shivering was in evidence but after the third hour a deficit began to appear hypothermia moved along progressively increasing the rate of descent with time

Talbott What determined the time of beginning observations following sympathectomy? Why did you select the second week versus the first week or the sixth week?

Keller When we first did these operations we did them for another purpose

Talbott Would the reaction be different as a function of time following sympathectomy?

Keller Yes In order to produce and demonstrate such a deficit it is necessary to remove both chains simultaneously or practically so If you remove one chain 2 weeks previously and the other 2 weeks later the deficit is not present or is not very striking Compensation occurs very rapidly and the compensation occurs without cold exposure stimulation

Fremont Smith Would this animal compensate after 2 or 3 months?

Keller That is right He would not show that deficit

Talbott This is a transient phenomenon in other words

Keller That is correct

Burton Could I ask whether this differs in dogs and humans? Barcroft (3) finds that it takes 9 months for compensation after sympathectomy in a human In the dog I see that in a couple of months there is compensation

Keller It depends on what the compensation is for These animals show permanent deficits in other realms Their insulin sensitivity is permanently lowered which demonstrates a permanent denervation of the adrenal medulla They permanently drink more water when subjected to a heat load which may be due to the absence of the vasodilators in the sympathetic chains They permanently exhibit a hypergastric secretory response to a hypoglycemic stimulus Therefore they exhibited permanent sympathectomy deficits yet they effectively compensated for a cooling load

Pace Have you measured epinephrine norepinephrine levels?

Keller No we have not

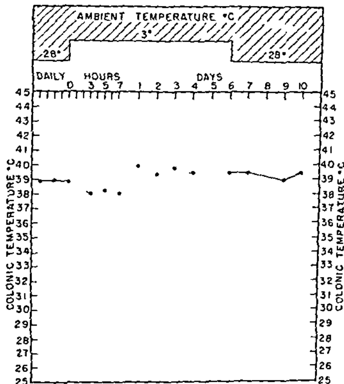


FIGURE 6 Core temperature response in dog 144 5 months after total removal of the vertebral sympathetic chains when abruptly subjected to 3°C There was no core hypothermia during the first 7 hours of exposure but there was a decided core hyperthermia subsequent to the first 24 hours of exposure

Figure 6 shows the extent of the compensation in terms of core temperature which obtained 5 months after sympathectomy in dog 144 Core temperature dropped from 39°C to the normal level of 38°C where it remained for the first 7 hours Then the following morning the animal was overcompensated to the neighborhood of 40°C where it was maintained for 6 days

Henschel Does this mean he maintains peripheral vascular control?

Keller We don t know what he has done In an initial attempt to determine the status of the compensated animal Lt Frieden a Medical Officer here at the Army Medical Research Laboratory who is interested in cardiovascular physiology investigated the pressor tachycardia response to cold in a few sympathectomized preparations Some of the animals Lt Frieden used were not verified com

plete sympathectomies He found a pressor tachycardia response to cold in these animals which was eliminated by adrenergic blocking drugs

This preliminary observation tentatively raises the question as to the possibility of cutaneous vasoconstrictors other than by way of the sympathetic chains I don t recall whether such a possibility has ever been suggested or suspected

Burton I think that the problem is present in humans According to Barcroft s (3) data on humans after a patently complete sympathectomy he tested patients by putting the legs in hot water to see if there was a reflex dilation and he found that after about 10 months there is a return to almost normal reactivity By what mediation this is done still remains a mystery It is very difficult to conceive of its being regeneration of the sympathetics after this type of operation

Keller After total sympathectomy the possibility of regeneration of cutaneous vasoconstrictor fibers is not feasible because the only fibers which remain to regenerate are the preganglionic fibers and they are cholinergic They will not connect and innervate adrenergic organs which the blood vessels are

Reynolds What about an axon reflex?

Keller That is entirely possible It is also possible but not probable that the prevertebral ganglia could serve as peripheral reflex centers as was formerly believed to be the case

Pace The adrenal medulla doesn t atrophy?

Keller No

Pace So you still have a functional adrenal medulla Perhaps it is a straight hormonal effect

Burton An axon reflex in humans is ruled out by this method of testing This is a reflex coming from putting the legs in hot water which dilates the vessels of the forearm

Lyman How long does that reaction take after putting the legs in water?

Burton It is claimed by some in England that they can see an effect within a few seconds

Fremont Smith Even after sympathectomy?

Burton No these workers haven t studied the time delay after sympathectomy in this particular set of patients The general reaction of dilation of the forearm is about 5 minutes after putting the legs in hot water

Fremont Smith It is not delayed as far as you know after the sympathectomy?

Burton No, and it is almost as complete as in an unsympathectomized person

Keller It is important to keep in mind the fact that most sympathectomies in humans are incomplete sympathectomies This must be total

Burch Kuntz (4) was able to identify sympathetic nerve fibers in the mixed nerves of dogs These may be compensating, don't you think so?

Keller I don't recall the details

Burch: He noted sympathetic fibers in the mixed nerves which originated in the central nervous system, outside the sympathetic chain

Keller That is the question we are raising I think it is worth considering and, of course, can be investigated in this type of preparation Kuntz was a very careful classical descriptive anatomist

In Figure 7, the previous two curves are superimposed, and the amount of compensation, or whatever occurs, is shown Although rather simple in type, these experiments demonstrate the tremendous compensatory potential of peripheral processes in this particular situation, the recognition of which should cause one to weigh carefully speculative conjecture based on fragmentary and unilateral information obtained from a single digit, appendage, organ or peripheral process

This is using the entire and intact animal, except for the one surgical variable and it illustrates the fact that the efficient homotherm is not as vulnerable to cold following peripheral denervations as we have been prone to assume

Horvath Dr Keller, one of the questions I have about this relates to the fact that apparently within 2 weeks after sympathectomy these animals had temperatures around 31°C, and during that entire time they shivered Theoretically somewhere around that time the shivering should have ceased Have you carried any of these animals down to, say 30°C and had their shivering stop?

Keller This particular animal had the most severe deficit we have encountered Out of the several animals observed, only those which were tested shortly after operation have shown a sizable deficit In our experience, shivering is definitely present in the unanesthetized dog at a core temperature as low as 28°C We have not investigated the level of disappearance of shivering However, Britton (5) reported shivering in cats at a 20°C core temperature

Horvath Somewhere around 30°C The point is, can the animal actually go down that far?

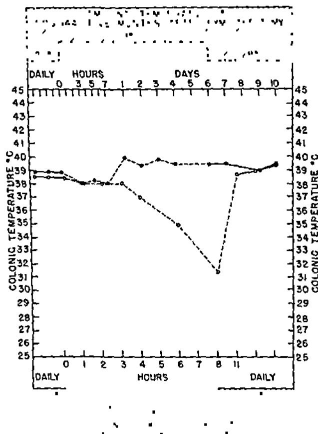


FIGURE 7 Superimposed core temperature curves shown in Figures 5 and 6 The marked improvement with postoperative time in this animal's resistance to a core hypothermia is readily evident

Keller If we had kept the animal in the cold longer, the core temperature certainly would have continued to fall Originally, when these experiments were done, we were still apprehensive about bringing animals down to low levels because we didn t want to lose them.

Montgomery Were any surface temperatures taken in this experiment?

Keller Not during the experiment

Montgomery Were skin or paw temperatures taken?

Keller No they were not The skin temperatures of course went up Blood pressure fell and was maintained at a slightly lower level permanently (6) The animals remain permanently vasodilated to a certain extent as can easily be determined by visual inspection of the vessels in the ear Also it is much easier to get into the vein and draw blood etc

Fremont Smith Did they overventilate?

Keller No not to my knowledge

Fremont Smith Humans generally overventilate when exposed to cold and the dog of course uses ventilation as a means of cooling If the dog were to overventilate in the usual way you would expect him to lower his body temperature This would be an adverse effect It would be rather interesting to know whether or not animals perhaps inhibit overventilation

Keller They don t pant not in the cold

Blair Panting and overventilation are different

Fremont Smith We both pant and overventilate Many years ago I studied this on myself by taking cold showers I also had myself lowered into a tub of ice water on a hot day overventilation was quite striking

Keller Some of these dogs resent the cold they don t like it These experiments are in some cases complicated by the fact that the dogs until they get accustomed to the cage walk around for a period of time

Montgomery For how long a time postoperatively were these dogs studied? Were they in excellent shape postoperatively?

Keller Yes We have had some of them in the laboratory for a year or so They showed no adverse effects of the sympathectomies as determined by clinical inspection

Davis Could you tell in which situation the animals shivered the most or did they shiver the same amount?

Keller Our measure of shivering has been by gross clinical inspection We can tell whether shivering is present or absent and whether vigorous or not very vigorous

Davis In which of these situations did the animal shiver the most the ones with the deficit or the ones that had compensated?

Keller I can t answer that question

Davis In other words here you have an important heat production method occurring in both cases and in one situation you are getting hypothermia

Keller Perhaps the compensatory mechanism is predominantly

an increase in heat producing ability But certainly the matter of increasing the ability to resist loss of body heat has also had time to occur In this regard we know that the smooth muscles in the blood vessels spontaneously regain some tone following denervation We also know that cold causes some vasoconstriction by stimulating the vascular musculature directly without any neurogenic influence

It has also been reported (7) that blood flow following sympathectomy is regulated by local tissue metabolites All these factors are no doubt involved But whether or not there is increased thermogenesis with time remains to be determined

Horvath I infer only one animal out of fifteen has successfully demonstrated this

Keller This is an experiment on one animal and any definitive conclusions drawn should be derived from the data presented in the figures showing this animal

Horvath Of those animals on whom you did perform an adequate or complete sympathectomy how many demonstrated a similar phenomenon? Is this a rather unusual animal the one exception to the rule or is this a typical one?

Another question naturally follows Suppose 90 per cent of the chain is removed and there is some small residuum somewhere perhaps at L 3 actually how much is this going to influence the magnitude of the cold response? Why should the loss or the presence of say only 1 per cent or 5 per cent of the sympathetic chain cause such a tremendous difference in the response of the animals?

Keller This animal is not unusual in any sense It showed the most pronounced hypothermic response of the animals tested because both chains were removed on the same day and it was tested sooner after operation than any of the others I cannot answer your second question To do so would require a large series of animals having identical graded extirpations and an identical time testing sequence This approach has not been accomplished

Pace It seems to me that you may be dealing with a threshold phenomenon You reach the point where the heat producing capacity of the animal to maintain body temperature is exceeded and then the body temperature starts to fall

Keller The compensation as demonstrated in this preparation was certainly a surprise to us and the how of it remains to be elucidated

Burton Could it be the crucial matter is the denervation of the adrenals in this operation? Have you tried comparing this with straightforward adrenalectomy?

Keller The adrenal medulla is *permanently denervated because* insulin sensitivity is reduced

Burton In all the animals?

Keller Yes

Burton So this would be the crucial point

Keller Yes insofar as determining the necessity of the innervated adrenal medulla In addition hypothermia is not precipitated under identical and even heavier cooling loads in the adrenalectomized animal if blood sugar is maintained

Taylor How much shaving of the animal was required at the operating procedure and how quickly does the hair grow back?

Keller There was a good deal of shaving because the chains are removed by three incisions on each side one dorsal abdominal in *cision and two thoracic incisions between the ribs So practically* the whole side of the animal was shaved in order to remove each chain It takes a matter of 6 weeks or 2 months I believe before the coat is entirely regrown

Bass You made a statement in passing that these animals were overdrinking in the heat Did you also notice whether during the first 2 hours before they became hypothermic any diuresis occurred? Are you following this by any chance? I know you are interested

Keller I don t recall having checked on cold diuresis in these par ticular animals The unoperated dog does decrease total body water by diuresing during the first several hours of cold exposure The diabetes insipidus dog also decreases total body water but in a differ ent manner namely by decreasing the amount of water drunk *while maintaining the same urinary volume*

Bass Have you any thoughts about the overdrinking in the heat?

Keller Yes It occurs to us that this may be a demonstration or verification of the existence of vasodilators in the sympathetic chains The existence of such fibers has been postulated for a good many years If this explanation is correct it would indirectly demon strate that the blood vessels cannot dilate maximally without vaso dilator impulses Therefore the animal increases its evaporative cooling to the extent heat loss by conductance is decreased due to *submaximal vasodilation* That *is one possible* explanation which is perhaps rather far fetched Another possibility is that thirst sati ation afferents taking origin from the gastrointestinal tract course in the sympathetic chains

Horvath The dog cools only evaporatively from the respiratory tract?

Keller Predominantly yes

Horvath I don t quite understand your logic

Keller He doesn t cool so much from his skin therefore he has to through his respiratory tract

Horvath How does that prove the presence of vasodilators?

Keller It doesn t prove it it s simply a possible explanation for the sympathectomized dog's increased drinking when exposed to heat

Burch Are you postulating that the degree of vasoconstriction in the surface is the same in both types of dog? A moment ago you stated chilling of blood vessel walls produced vasoconstriction

Keller That has been demonstrated in the isolated limb of the dog (8)

Burch Would you say the vasoconstriction in the two sets of dogs was the same?

Keller No The explanation specifies a submaximal vasodilation in the sympathectomized dog whereas it would be maximal in the unoperated animal These animals must be studied in the calorimeter of course to answer all of these questions Otherwise the answers must remain speculative conjectures

Travell Isn t it true that the sympathectomized and the control dogs differ in that the sympathectomized group had already experienced a previous exposure to cold loading? How long was the interval between the two exposures?

Keller These sympathectomized dogs were not exposed to cold previous to sympathectomy

Horvath This was the same dog

Travell It is the same dog that was previously exposed so it is not the same as the control

Fremont Smith You couldn t have gotten the previous experiment

Keller Not previous to the operation

Fremont Smith Not immediately previous but previous to the operation

Travell Could this be merely a variable from one dog to another?

Keller No Some of the sympathectomized dogs do not show hypothermia without any postoperative cold exposure if sufficient postoperative time is allowed to elapse The recovery from the sympathectomy so far as the animal s ability to prevent hypothermia is concerned occurs spontaneously without cold exposure

Montgomery Do any of the dogs not sympathectomized show any proportion of this amount of fall in body temperature in exposure to cold?

Keller A healthy unoperated dog never shows this body temperature fall under the cooling loads illustrated Following other types of operations, they do, as we shall now demonstrate Next, I shall discuss the second surgical variable, that of hemisection of the brain stem at the level of the pons (Figure 8)

It has long been known that the physiological resistance to hypothermia, as demonstrated in Figures 1 and 2, is due to integrative activity of the central nervous system as evidenced by its complete disappearance in the absence of certain localized parts of the central nervous system (9, 10 11)

Figure 8 describes an experiment involving two surgical variables that Col Blair and I did years ago (12) The first surgical procedure was a right hemisection of the brain stem at the level of the pons and the second complete transection of the brain stem accomplished

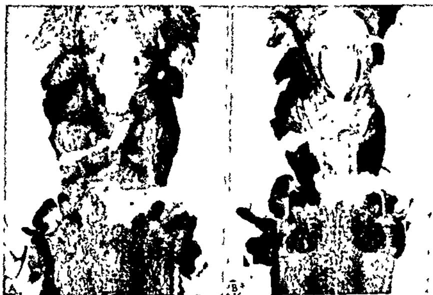


FIGURE 8 Frontal Pal Weigert sections taken from the series on a dog in which *opposite hemisections of the brain stem were placed at different levels* (*A*) is a section through approximately the middle of the brain stem The lower and right hemisection passed through the caudal extent of the pons and in addition crossed the mid line considerably to involve the medial half of the left side of the brain stem The upper and left hemisection passed through the cephalic portion of the midbrain and involved the medial aspect of the right side of the brain stem only slightly (*B*) is a ventrally situated section in which the superior olives and substantia nigra are prominent localizing anatomical structures Reprinted by permission from Keller A D and Blair J R Further observations on distribution at level of pons of descending nerve fibers subserving heat regulating functions *Am J Physiol* 147 500 (1946)

by a left hemisection executed months later at the level of the mid brain

For the present I would like to discuss the frontal section on the left (*A*) The first and lower hemisection on the right lies at the cephalic level of the medulla The hemisection was made by inserting a No 8 milliner s needle from above downward in the mid line and pulling it laterally The path of the needle is shown by the straightness of the cephalic edge of the tissue defect

The caudal enlargement of the needle path was caused by an anemic infarct resulting from the disturbed blood supply The tissue defect comprises more than a hemisection it crossed over to the left side considerably So it was roughly a three quarter transection of the upper medulla Then at a subsequent operation the transection was made complete by an opposite hemisection but at a higher level of the brain stem so that we would know and could describe the lesion path in each instance

Figure 8 *B* is another section from the series on the same dog showing the two hemisections at a more ventral level at the level of the pyramidal bundles These bundles were completely severed Thus here is a completely verified total transection of the brain stem except there was a possibility that fibers might descend through the transection by decussating between the two hemisections That would be the only possibility of fibers getting through

The cold exposure runs on the dog shown in Figure 8 following the first hemisection are shown on the left hand side of Figure 9 One week after the first hemisection when this animal was subjected to the environmental test which in this case was 10°C body temperature fell progressively from 36° to around 30°C in 2 hours there was no shivering We were using in this instance rather than a walk in cold room a large refrigerator

Four weeks after the first operation the body cooled about as rapidly as it did in the first run for the first hour but at a core temperature of 34°C shivering was elicited and cooling was subsequently less rapid And in 6 weeks the animal s physiological resistance to hypothermia was nearly normal

The questions arises here Was the animal s change in status from the nonregulatory state obtaining 1 week after operation to the essentially normal state 6 weeks after the brain stem hemisection due to returning function of unsevered heat regulating nerve fibers or to the progressive activation of a subsidiary medullary center? There has long been speculation as to whether or not a subsidiary medullary center for cold combatting powers exists Sherrington

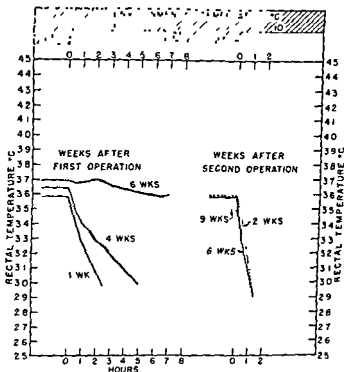


FIGURE 9 On the left are shown cooling curves obtained following the right hemisection shown in Figure 8 Progressive recovery of resistance to hypothermia is evident from the first through the sixth postoperative week after the three quarter transection through the caudal extent of the pons Cross marks denote the presence of shivering On the right side are shown cooling curves obtained following completion of the transection by the higher and opposite (left) hemisection There is a total absence of any recovery in resistance subsequent to completion of the transection Rearranged and printed by permission from Keller A D and Blair J R Further observations on distribution at level of pons of descending nerve fibers subserving heat regulating functions *Am J Physiol* 147 300 (1946)

(9) demonstrated in a convincing manner that there was no such subsidiary spinal mechanism

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lateral one fourth of the brain stem which were temporarily de functioned, as we know occurs in traumatized but unsevered periph eral nerves

These questions were answered in a crucial manner by the third

surgical variable, namely, completing the brain stem transection by placing an opposite and second hemisection

There was no recovery of physiological resistance with postoperative time

Thus, to determine the status of a deficit inflicted by a central nervous system lesion at least 6 weeks must elapse before an evaluation is begun If a mechanism is intact following an acute lesion that is a different matter A conclusion can then be drawn immediately

This experiment verifies and conclusively demonstrates that the physiological resistance against cold is integrated at a level higher than the midbrain, or stated in another way, physiological resistance to hypothermia is absolutely dependent upon a pre midbrain neural integration

It should be emphasized that there are no compensatory responses ... to ... as

The sympathectomy data were purposely inserted into this presentation to demonstrate the difference between a deficit which remains fixed and one which, in a sense, refuses to stabilize Great care must be taken in drawing conclusions from a deficit which fails to remain reasonably static

Talbott Did you test this 1 or 2 years later in any of the animals?

Keller Only weeks later as in the case history just presented These are brain stem transected animals and a considerable amount of nursing care is required to keep them alive

Talbo... been able to do it?

Blair ... 17 or 18 years ago, we studied ... complete We studied them later than 2 weeks postoperatively Some of them we tested many months after this and there was no return after the second operation

Fremont Smith Figure 9 shows 9 weeks

Blair We had other tests after this that followed the same pattern as these animals

Keller That was a further and important reason for doing the transection operation in two stages namely by two successive hemi

sections An animal after a three quarter transection can be maintained indefinitely with relative ease we have for instance kept such preparations for as long as 9 months Therefore the tissue lying caudal to the first hemisection is long fully chronic at the time the second hemisection is placed Had the recovery shown by the curves on the left been due to the activation of a subsidiary mechanism then the second hemisection would not have occasioned even a transitory deficit

Fremont Smith What is the interval between the two operations?

Keller Between the two hemisections in this particular experiment 8 weeks elapsed

Horvath How much weight had the animals lost in this process percentage-wise?

Keller They lost considerable weight during the acute postoperative period

Blair They gained most of it back

Horvath The first one is incomplete so they have a chance to recover

Keller These recover well and regain their weight fully with time In answer to that question Doctor H L Batsel (13) has three or four such completely transected dogs They all go into negative nitrogen balance It is difficult to get them on full feed because of the depressed swallowing reflex and a tendency to regurgitate food if the stomach is filled a little too much Most of them are lost however with hypostatic or aspiration pneumonia They must be massaged at frequent intervals so it is predominantly a 24 hour nursing care problem

Pace Is the reduced body temperature at normal ambient temperature after the second operation characteristic of these preparations?

Keller The dog is a homotherm and as will be shown later the difference between the ambient or environmental temperature and body core temperature is usually about 8°C if the dog is in good physical condition

The term that Isenschmid and Schnitzler (14) used to designate the difference between air and core temperature was regulation interval

Montgomery Are the completely transected dogs kept in a carefully regulated temperature? If so what?

Keller They are kept in a warm environment but we have learned it is not necessary to be too careful about this

Montgomery Would a 20°C room not allow for any fall in core?

Keller We keep them around 28°C the core temperature then

being in the neighborhood of 36°C. Core temperature does not worry us except it must not be above the homothermic level. We try to keep the core temperature around 35° to 37°C.

Horvath: Although I don't doubt the importance of the nervous system in this regulation, I wondered if at any time you had taken an animal and kept him immobile after the second double transection. If you kept a control animal for 9 or 10 weeks in the same immobile state, how much of a response pattern, which would be similar to this, would be present? How much of this is actually due to relative loss of body weight?

It has been known that if animals are put in a small cage so that they can't move around, they will become hypothermic. A rabbit does this, the rat does this, and several other animals. I don't know whether the dog does it or not.

Keller: I think of the rat and even the rabbit as being partial homotherms as compared with the dog. However, I suspect body size and emotional reactions associated with restraining are important factors in this situation. I don't know how you would immobilize an unoperated dog without anesthesia. Of course, anesthesia immobilizes heat regulation.*

Horvath: These all have indications of immobilized temperature regulation. Your temperature drops and it was generally lower than it was at 2 weeks.

Fremont-Smith: Whether or not this could be a result of the secondary immobilization is an interesting point.

Keller: This animal was completely atonic for 9 weeks following the second operation. It was the type of atonicity supposed to occur only after bulbar section. But if you do them higher, they remain completely atonic and reflexes are depressed.

Horvath: Dr. Henschel suggests a little curare.

Keller: What for?

Horvath: For this immobilization.

Fremont-Smith: To immobilize the normal dog.

Keller: We know from Claude Bernard's original experiment that body temperature falls under curare.

Davis: For the same reason this does.

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*Editor's Note: Dr. Keller would like to add the following afterthought to his remarks at the Conference:

In retrospect, a dog can be completely immobilized by a central nervous system lesion without materially impairing its resistance to hypothermia (1). Also the dog can easily be trained to be motionless except for the shivering. There isn't any question that physiological resistance to hypothermia in the dog is completely independent of spontaneous muscular movement.

Keller The principal effect of curare is to remove shivering isn t it?

Davis No the movement can be knocked out entirely

Keller Yes

Davis You immobilize the same way you are here

Keller Yes but under curare the animal will rewarm himself after cooling while he is still immobile The experiments done at the Naval Laboratory recently by Werner (16) demonstrated that in the dog Therefore the curarized dog does have heat producing ability independent of visible muscular movement

Blair Metabolism regulates the ability to shiver and to resist most severe cold exposure

Burch Do you have any data on heat production in these dogs?

Keller Yes They do not have a lower basal energy metabolism

Burch Their oxygen consumption was unchanged?

Keller Their oxygen consumption is normal at the homothermic core temperature

Burch What about in the cold?

Keller Oxygen consumption falls progressively as body temperature falls

Burch Does the rate of heat production decline with oxygen consumption?

Keller Yes

Behnke Do the dogs lose their sensitivity to insulin after this second operation?

Keller Insulin sensitivity is not materially altered

Hock I would like to point out that the lowest rectal temperature occurring here is about the same as that which I find in bears in the winter I have often wondered whether or not the fall of temperature displayed by bears in winter is due merely to immobility or perhaps to the more profound causes that appear to operate in species that attain deep hibernation The bear goes in his den and lies down for practically the whole winter My captive bears are kept as nearly like wild bears as possible They lie still for long periods of time and appear to move about only once a day or less

The temperature shown here is nearly the same as the lowest winter rectal temperature I have recorded which is 31 2°C Active bears have rectal temperatures close to 38°C at any time winter or summer without respect to the air temperature When they are immobile in the winter dens they have a reduced temperature (17)

Lyman Do they shiver when they are immobile?

Hock I have not observed shivering in the immobile state

although shivering is apparent when the bears are still active in temperatures of $-35°C$ or below

Keller Exercise is the most potent thermogenesis we know about

Horvath Isn t there sometimes an accident which prevents your holding the temperature? What is the temperature after 2½ hours? Does the temperature keep going down precipitously?

Keller If you let body temperature go any further you lose them soon

Horvath How long

Keller We have never investigated that simply because it has taken too much work to keep them and we ve never wanted to run chances of losing them

Blair We always kept a hot box ready where we could quickly rewarm them The critical point was 30°C because we found by experience that was the point where we stood a good chance of losing the animals Occasionally it may have gotten below that but we quickly rewarmed them They certainly have gone down lower After 4 weeks they were shivering As their temperature became lower they continued to shiver at no greater rate and would have died I am quite sure

Keller We have lost some through carelessness on the part of the person responsible

Horvath How did they die of fibrillation?

Keller We are reasonably certain that some of them fibrillate (1) but whether some can be taken down to asystole has not been determined The fourth surgical variable is the matter of sectioning the nerve fibers that take origin and descend from the hypothalamic level or above (Figure 10) We were interested to know whether severing the descending fibers exiting from the hypothalamus would reduce the animal to the completely non heat regulating status This is one of the first things we accomplished in this laboratory after we had appropriate facilities for attempting the experiment Figure 10 shows a sagittal Pal Weigert section taken from the series on dog 28 and illustrating the surgical extirpation which we term a posterior hypothalamectomy (13) The tissue defect was accomplished with the coagulating current from a Bovie electrosurgical unit administered through a ball electrode introduced through an opening in the pia at the caudal extent of the mammillary bodies

Fremont Smith Would you orient us a little on this?

Keller The optic chiasm lies ventrally on the left The thalamus is dorsal the habenular tract separates the thalamus and hypothala



FIGURE 10 A sagittal Pal-Weigert section taken from the series of dog 28 illustrat the coagulated tissue defect which is characterized as a posterior hypothalamectom The large tissue defect is relatively free of scar tissue and is in the location previous occupied by the posterior half of the hypothalamic gray while the ventricular syste has remained intact and undistorted Reprinted by permission from Keller A D Hypothermia in the unanesthetized poikilothermic dog *The Physiology of Induce Hypothermia* National Academy of Sciences Publication 451 1956 (p 61)

mus from the midbrain which is on the right the anterior commis sure is above the chiasm and the corpus striatum is to the left The anterior hypothalamus and the pituitary gland are undisturbed The large tissue defect is relatively free of scar tissue forms an enlarge ment of the ventricular system and is located in the space previously occupied by the posterior hypothalamic gray mammillary bodies and the portions of the fornices and mammillothalamic tracts which course through the posterior hypothalamic gray

Figure 11 is a cross section taken through a similar tissue defect from the series on another animal dog 35 The thalamic infarcts which are visible dorsally complicated the operation in this animal the extirpation in dog 28 was uncomplicated Here also the pituitary was not disturbed and the posterior half of the hypothalamic gray was coagulated as in dog 28

The data in Figure 12 illustrate how such preparations respond to the test situation of 3°C First it was necessary to maintain the

animals at an ambient temperature of 30°C, roughly, to keep the body temperature at 38°C. When they were placed in a 3°C room body temperature fell in a straight line, reaching 28°C in 3 hours. These two preparations were identical. There was no shivering. When they were removed from the 3° to a 22°C ambient temperature, they maintained their body temperature at 28°C without any spontaneous rewarming. External heat was necessary to rewarm them. With a core temperature of 28°C, the difference between environmental temperature and body temperature was 6° rather than the 8°C differential which obtained when core temperature was 38°C. This is a demonstration of the Q 10 effect.

Horvath: In the record of temperature you show data at 8 hours and suddenly another point at 24. Is the implication that the temperature rises at that slow rate?

Keller: No.

Horvath: How long does it take to recover?

Keller: I don't know precisely. We recorded temperatures the following morning. That is all the data indicate. Of course it depends upon how rapidly the animals are reheated. They rewarm



FIGURE 11. A cross section Pal Weigert section taken from the series on dog 53 which also illustrates a posterior hypothalamectomy. Most of the tissue defect by virtue of its being free of scar tissue constitutes an enlargement of the third ventricle. The pituitary gland and particularly the stalk remain intact and undistorted.

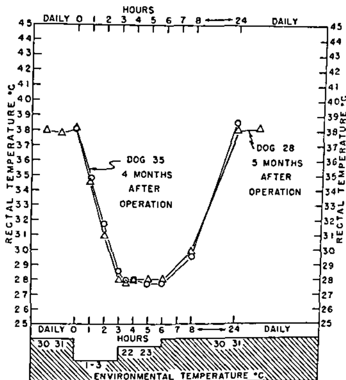


FIGURE 12 Cooling curves observed during subjection to a 3°C. air temperature in the two posterior hypothalamectomized animals illustrated in Figures 10 and 11 Incubation at 30°C is necessary to maintain a 38°C core temperature There is a straight line fall in core temperature from 38° to 28°C in 3 hours and there is a regulation interval of 8°C at a core temperature of 38°C, and of 6°C at a core temperature of 28°C Reprinted in part by permission from Keller A D Hypothermia in the unanesthetized poikilothermic dog *The Physiology of Induced Hypothermia* National Academy of Sciences Publication 451, 1956 (p 61)

rapidly in the 30°C, and there is no danger of overheating, so we are not concerned

Horvath The only point I wish to make is that there is a suggestion of a difference in the recovery time The curves as presented indicate some differences in the cooling rates against the rewarming rates, and this would suggest changes in terms of heat regulation which we can t, of course, get from Figure 12

Keller The only part of the curve that you could use there would be between the 6 and 8 hours That would give you the rewarming rate The other was just a matter of the 24 hour reading

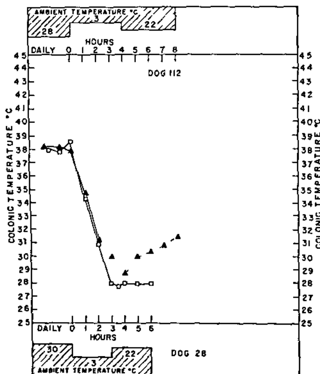


FIGURE 13 Cooling curve for dog 112 1 month after partial hypothalamectomy There was the same straight line fall in core temperature as exhibited by the control complete poikilotherm until at a core temperature of 31°C shivering was activated ▲ = retention and isolation of remnantal shivering
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Henschel Have you calculated from metabolism body surface relationships what this curve should be theoretically?

Keller No, I have not Dr James D Hardy, of the Department of Physiology at the University of Pennsylvania School of Medicine, is observing some of these preparations in the calorimeter He will no doubt report precise information in this realm

The data in Figures 13 to 19 illustrate the cooling curves obtained when varying amounts of posterior hypothalamic gray tissue escaped being coagulated In Figure 13, the curve with the open squares constitutes the control curve, *i e*, the status when all resistance to hypo

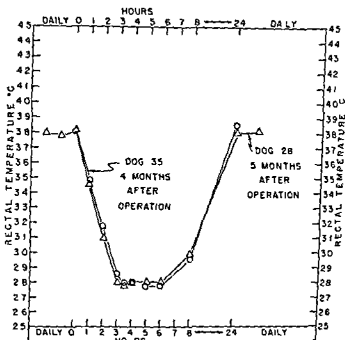


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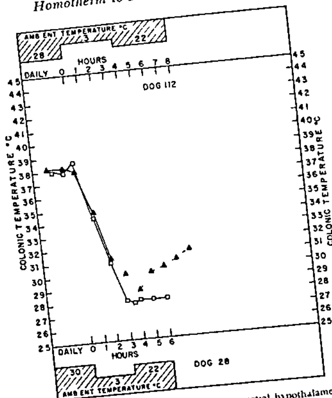


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The data in Figures 13 to 19 illustrate the cooling curves obtained when varying amounts of posterior hypothalamic gray tissue escaped being coagulated In Figure 13 the curve with the open squares constitutes the control curve *i e* the status when all resistance to hypo-

thermia is removed, and the closed triangles constitute the curve obtained from an animal that retained a remnant of resistance It cooled at the same rate as the complete poikilotherm, as we arbitrarily speak of them to a core temperature of 31°C, when shivering was activated, after which the curve moved to the right Also, when the animal was moved to a 22°C room, it spontaneously rewarmed The heat produced by shivering was readily evident Physiologically, this delayed activation of shivering at a markedly reduced threshold (31°C) fascinates us These preparations are ambulatory, do not need special nursing care other than a warm environment and the particular deficit obtaining in each preparation remains stable Therefore, features such as this lowered shivering threshold can be investigated

Fremont Smith Do they change the threshold for shivering?

Keller No, as they warm up, shivering stops at the core temperature at which shivering was activated while they were cooled

Lyman Would that level off at 31° or 32°C, then, and would they continue to shiver?

Horvath Theoretically, they would have to stop

Keller If they stopped shivering, it would level Figure 14 gives simple illustrations Here, in Figure 14, an animal cooled progressively from 37° to 27 5°C without shivering but it took 5½ hours whereas the complete poikilotherm cooled in 3 hours This animal also rewarmed spontaneously in the 22°C room temperature

Henschel Were the animals the same size?

Keller Yes, relatively the same size The question arises How was the rewarming accomplished? Col Blair and I (18) encountered this particular type of preparation for the first time several years ago We did not have calorimetry or oxygen consumption apparatus available at that time, but we interpreted this slower cooling and spontaneous rewarming as evidence that the animal had the ability to produce cold stimulated nonshivering heat It was reasoned that if the slowed cooling was due to a slower rate of heat loss by conductance than in the other animals rewarming should not occur

Oxygen consumption data on these animals have verified this conclusion (1) An example of this type of evidence is shown by Figure 15 On the left is the control cooling curve for the complete poikilotherm dog 447 Temperature points are marked with crosses on the cooling curve for dog 396 It is evident that after core temperature reached 35°C the curve moved decidedly to the right there was no shivering Calories per kilogram of dog per hour are

charted at the top of the figure, reading from right to left Heat production at a given core temperature *is shown by open* bars Heat production plateaued as the cooling curve moved to the right, thus, the animal cooled more slowly than the complete poikilotherm because it possessed a remnantal cold stimulated heat producing ability

Burch Is the source of the heat from muscle or liver?

Keller We do not know We are careful to say nonshivering heat rather than nonmuscular heat

Burch Have you any idea what it is?

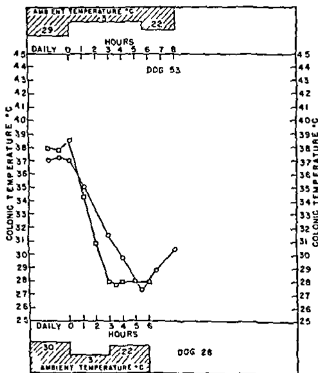


FIGURE 14 The cooling curve for dog 53 4 months after partial hypothalamectomy (open circles) is contrasted with the curve of a complete poikilotherm (open squares) There is a straight line fall in core temperature but at a slower rate than in dog 28 There was no shivering thus in this instance there was a retention and isolation of remnantal nonshivering thermogenesis Reprinted in part by permission from Keller A D Hypothermia in the unanesthetized poikilothermic dog *The Physiology of Induced Hypothermia* National Academy of Sciences Publication 451 1956 (p 61)

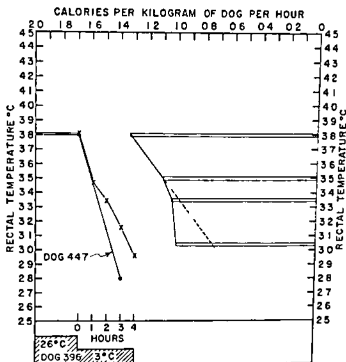


FIGURE 15 Cooling curve and heat production values for different body temperatures graphed as bars for dog 396 evidencing retention of nonshivering heat producing ability and a definitely raised threshold (lower body temperature) for the activation of the nonshivering heat elaborating process Reprinted by permission from Keller A D Hypothermia in the unanesthetized poikilothermic dog *The Physiology of Induced Hypothermia* National Academy of Sciences Publication 451 1956 (p 61)

Keller Yes, we think the liver is involved, that at least some of the cold stimulated nonshivering heat is produced in the liver However, our evidence is too limited to make a positive statement We feel that the way to demonstrate crucially that there is such a thing as cold stimulated nonshivering heat production is to determine where and how it is produced It is hoped this can be accomplished with this type of preparation

Fremont Smith Do you have electromyographs to show it was not invisible shivering? You say, nonshivering You might have a subclinical shivering which you couldn t see without an electrical re cording

Keller That is correct, but subclinical or preshivering electrical activity always breaks into visible or palpable shivering if the cooling

stimulus continues and particularly if the stimulus is intensified Accordingly it seems reasonable not to suspect invisible or non palpable muscular contraction as the heat source in such cooling curves as shown in Figure 15

Davis Why do you discount muscle entirely? It is quite a large organ

Keller I don t discount muscle That is the reason we have always used the term nonshivering rather than nonmuscular

Davis I mean as a nonshivering heat producer

Keller If it is muscle tone? Muscle tone could be a contributing factor in cold stimulated thermogenesis to a limited extent

Davis You can pick up muscle tone electrically You can demonstrate this is not present either

Keller I would still call that preshivering electrical activity wouldn t you? I don t believe it is possible to distinguish between muscle tonus electrical activity and preshivering electrical activity during cold exposure

Davis No but it can be demonstrated that it is not present Therefore there is no shivering

Keller If you fail to obtain a record of electrical activity you say there is no electrical activity providing my apparatus is working or is sufficiently sensitive

Davis It is rather a large organ to become suddenly inert when it stops shivering

Horvath What contribution is made by the muscle mass to the total heat production?

Davis I would like to know that myself for both the shivering state and the nonshivering state

Keller You never know when curare is working maximally and it is known to have effects other than on muscle It is seldom that a chemical agent used as a physiological tool yields a crucial answer

Pace The Qo of muscle tissue is perfectly respectable when compared to that of other tissues

Davis The Qo of muscle certainly rises in the nonshivering state I do not know whether you can draw the conclusion that it is a heat producer or not I was just questioning your discarding the idea that muscle is not capable of producing heat when it is not shivering

Keller I don t discard the idea I simply do not speculate

Pace Do you postulate in these two different partial hypothalamectomies that you have destroyed different portions in one case

the shivering center and in the other, the nonshivering center?

Keller Yes We postulate there are separate and distinct systems of nerve cells and tracts for shivering and for cold stimulated non shivering thermogenesis Remnants of the two systems can be physio logically dissociated both at the hypothalamic and low brain stem levels When shivering is eliminated and nonshivering heat pro duction is evident, we feel that all the shivering cells are absent and a few nonshivering heat producers remain intact

Davis I think the first experiments you presented in Figures 13 and 14 demonstrated nonshivering thermogenesis perhaps a little better, in that there was shivering in both instances presumably of an equivalent amount In one case, body temperature was main tained and in the other, it was not

Keller Yes I would estimate that this animal retained a larger remnant of nonshivering heat producing cells than did the second one shown with accompanying heat production data That is right

Burton Careful wording is important here There must be a clear distinction between 'nonshivering" and 'nonmuscular" There is activity of muscle, thermal tone, as we called it many years ago which does not exhibit itself in the tremor in response to cold, which is the definition of shivering So we must be careful when we say nonshivering' that we don t necessarily mean "nonmuscular'

Davis I think Dr Keller and I are both careful

Burton In the conversation, I was confused whether or not you were making the distinction In one case, there is thermogenesis which is extramuscular That is quite a different question, whether the muscle may still be producing extra heat in response to cold by muscular activity, but not reaching the degree of coordination of the units which give the tremor

Taylor I should like to ask in that regard, if Dr Davis will ex plain what he means by saying the Q_{O_2} goes up in nonshivering muscle

Davis When an animal is acclimatized, shivering disappears and it disappears completely in most animals, but there still is increased heat production in response to cold exposure This is true in the rat it is partially true in man

Keller It is also true that shivering becomes much less promi nent as cold exposure is prolonged in the unoperated dog

Davis I think it is true in most homothermic animals

Taylor You are talking about the temperature quotient in the intact animal?

Davis This is *in vitro*

Pace It is conceivable that there may be increased mitochondrial activity quite apart from the actomyosin system These two don't have to be related I don't see any problem in visualizing an increased heat production by muscle quite independent of its mechanochemical coupling system

Davis I don't quite agree with Dr Burton that we can explain this away by a tone that we cannot pick up as a tremor I myself don't think this is demonstrable

Behnke It can be picked up by measurement of electrical activity in the muscle If a pillow is removed from under the head of an individual whose basal metabolism is being measured while he is lying comfortably on a cot then muscles are put on a stretch and metabolism will increase 25 per cent This increase in metabolism is accompanied by a marked increase in recorded electrical potentials

Davis This can be picked up As I interpret Dr Burton's statement this exists and it cannot be picked up at all on the EMG

Burton No I meant it exists it can be picked up with the electromyograph I base this on my old work with Bronk (19) Long before the state of muscle activity which exhibits tremor is reached the electrical activity increases We studied the mechanism how the motor units were cooperating or failing to cooperate being out of phase and found that the eventual shivering *i e* the tremor was just the last stage of this process of increased activity of different units out of phase

Davis It is measurable you admit it is measurable?

Burton Certainly yes

Keller If there isn't such a process as cold stimulated extramuscular thermogenesis and two separate and distinct mechanisms exist within striated muscle it will be extremely difficult to demonstrate their separate existences

Burch You said you thought the liver was one of the organs

Keller Yes the splanchnic area

Burch What is your evidence for that?

Keller Our evidence is as yet entirely too scant to be certain The approach is the determination of oxygen consumption in the splanchnic area in the type of preparation described above (20)

Burch Was oxygen consumption noted to rise in the studies you have done so far?

Keller We think so but our data are too preliminary to comment further

Davis It does rise

Keller The liver, of course, is an organ that has been suspected on the basis of presumptive data since Claude Bernard

Davis People maintain heat during sleep

Horvath Werner (16) showed some of this a few years ago

Keller The liver *is* a *logical organ to suspect as a nonshivering* heat source but other tissues, perhaps all tissues, could also be utilized for this purpose

The data in Figure 16 illustrate, in a partially hypothalamectomized preparation, a delayed type of deficit During the first 3 days of exposure, the animal did not develop much hypothermia but during subsequent days, the deficit became prominently apparent A sizable amount of hypothalamic gray remained intact in fact the lateral one fourth of the posterior hypothalamic gray remained undisturbed If an animal does show such a deficit when assessed 3

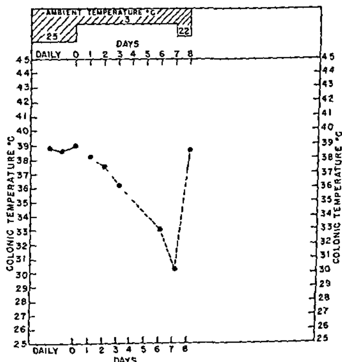


FIGURE 16 Cooling curve for dog 48 3 months after partial hypothalamectomy demonstrating a deficit in this animal's physiological resistance to core hypothermia characterized by its relatively slow onset to uncover the deficit it was necessary to subject the animal to the cooling load for days rather than for hours The lateral left half of the posterior hypothalamic gray remained undisturbed in this animal

months after the operation the deficit is permanent It is there for as long as the animal lives There is no compensation such as that which occurs after sympathectomy

Burch Figures 15 and 16 are interesting Isn t that a phenomenon of fatigue? Do you think the compensating mechanisms wear out and exhaust the stores of energy?

Keller I don t know Either a fatigue of neurons in the central remnant or inadequate peripheral fuel supply would fit into a speculative explanation However the latter does not seem probable because these deficits can be precipitated without discernible associated endocrine hypofunction

Burch It could be something quite different Were they eating?

Keller Yes The animals the first day are without food At the end of 8 hours we feed them The next morning and subsequently they are fed daily as per pre exposure routine

Burch Did oxygen consumption change with time also?

Keller We have not measured oxygen consumption in this type of preparation

Burch Some mechanism seems to break down rather quickly

Keller Obviously but it doesn t break as fast as in the animal having a more severe deficit It is just a matter of severity of the neurological deficit inflicted by the hypothalamic tissue defect This type of deficit would be missed entirely by a 6 hour exposure That is the reason it is important to prolong exposures into days if a deficit does not become definitely evident in a few hours

Fremont Smith The animal must be kept under stress long enough for whatever compensation mechanism he has to break down

Keller We do not believe that compensation in its true sense is involved rather it is an inadequacy of the remaining groups of cells because after sufficient postoperative time elapses for the edema and neighborhood sequelae to clear the deficit remains permanent and does not change in magnitude thus no compensatory responses appear to be involved in this situation

Burch Do you have data on steroid excretion?

Keller No these preparations are never thrown into an adrenal insufficiency crisis by cold exposure or by a profound hypothermia Also cold exposure does not precipitate hypothermia in the adrenalectomized dog if blood sugar is maintained Blood sugar falls fast without therapy and hypothermia develops after the hypoglycemia becomes profound A hypoglycemic crisis will precipitate adrenal

insufficiency in the adrenalectomized or hypophysectomized dog but cold exposure or hypothermia *per se* does not

The data in Figure 17 demonstrate another type of deficit encountered following a large hypothalamic tissue defect Core temperature dropped to 35°C the first day but held at this level for the remaining 23 days a profound hypothermia didn t break through When taken out of the cold room core temperature returned to or toward normal

Figure 18 illustrates a still different type of deficit This dog ran a hypothermia of 2°C when housed in a neutral environment When placed in the cold room its core temperature returned to normal where it was maintained for the duration of the exposure When returned to the neutral environment the 2°C hypothermia returned Figure 19 will illustrate the lesion Most of the anterior hypothalamus is absent here whereas the posterior hypothalamus

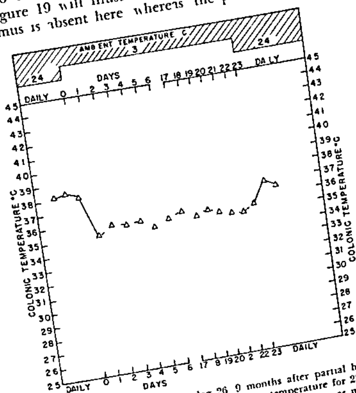


FIGURE 17 Core temperature curve for dog 26 9 months after partial hypothalamectomy before during and after exposure to a 3°C air temperature for 23 days There was immediate lowering of colonic temperature to 35°C where it was maintained for the duration of the exposure

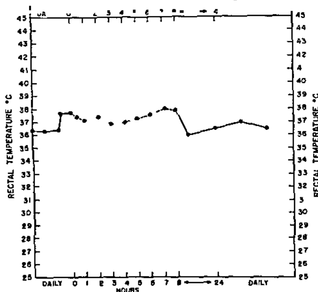


FIGURE 18 Core temperature response for dog 36 4 months after an essentially total anterior hypothalamectomy had been inflicted There was mild hypothermia when the dog was housed in a neutral environment whereas exposure to a 3°C air temperature caused core temperature to be elevated to the normal homothermic level

is intact This constitutes a near total *anterior hypothalamectomy* and the fifth surgical variable On the basis of this type of tissue defect we had previously concluded that as long as the posterior hypothalamus is intact the animal s regulation against cold would not be materially impaired

Horvath Posterior hypothalamectomized animals must have the environmental temperature at 31°C

Keller At 28° to 30°C

Horvath What happens to this type of animal if he is placed at 30° or 31°C and then put in the cold? In other words will his rectal temperature be nearer 37°C?

Keller Such an animal cools very rapidly

Horvath If he starts at a higher temperature does he cool down if he is at a lower ambient temperature?

Keller If the animal has been subjected to heat loading just



FIGURE 19 A sagittal Pal Weigert section taken from the series on dog 36 The tissue defect is located in the area previously occupied by the anterior half of the hypothalamic gray The posterior half of the hypothalamic gray lying just cephalad to the mammillary bodies and mammillothalamic tracts remains intact

previous to being placed into the cold, cooling is much more rapid than when it is placed in the cold from a neutral environment There is a lag in changing from a status of vasodilation to vasoconstriction, etc

The cooling curve shown in Figure 20—a definite and characteristic deficit—was obtained in a dog inflicted with the sixth and last surgical variable Obtaining such a deficit following the surgical extirpation to be described was new in our experience and, on the basis of the conclusion mentioned above, was entirely unexpected Yet, in retrospect it is not surprising in view of older literature on localization of heat regulatory functions in the central nervous system

Figure 21 shows a sagittal Pal Weigert section taken from this dog s series The tissue defect is clearly cephalad to the hypothalamus It is located in the space previously occupied by the preoptic area and the tissue which lies ventral to the corpus striatum I was

taught that the heat regulating center was located in the corpus striatum. Barbour's (21) work of warming and cooling the brain base influenced this localization to a considerable extent.

I remember an enjoyable conversation with Barbour before he died. I said, "Why did you make the mistake of saying the sensitive elements for heat and cold were in the corpus striatum rather than in the hypothalamus?"

He said, "Well, it was because I didn't know any neuroanatomy. I am not an anatomist. I had to rely on my friends. I apparently consulted the wrong people. (Laughter) I at first spoke of warming and cooling the base of the brain. Then my anatomist friends said, 'You should be more specific.' If I had stuck to the original wordage, I wouldn't have gotten into trouble."

In a sense, the data just presented comprise a verification of Barbour and others associating the area of the corpus striatum with heat

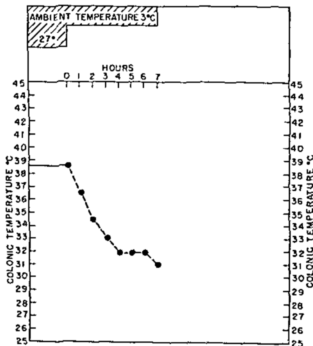


FIGURE 20. Core temperature response to a moderate cooling load (3 C) in dog 303 3 months after he had been inflicted with a large coagulated tissue defect involving the whole of the preoptic area. The entire hypothalamus proper remained undisturbed.



FIGURE 21 Sagittal Pal We gert sections taken from the series of d g 303 wh ch exhibited the def cit in resistance to core hypothermia illustrated in Figure 20 (A) is fro n near the mi ll ne an l (B) is from a lateral location through the plai e of tl e mammillothalamic tract The entire hypothalamic gray an l tl e pituitary gland remain undisturbed whereas the tissue defect lies cephalad to the opt c cl iasm and immediately dorsal to the optic nerves

regulation because the lesion lies ahead of the chiasm and ventral to the corpus striatum the hypothalamus remains undisturbed

Figure 21 reveals that both the hypothalamus and pituitary are undisturbed The tissue defect lies entirely cephalad to the hypothalamus having extirpated the prechiasmal area

Figure 20 shows the striking deficit exhibited by this animal We do not know the underlying physiological basis for this deficit but obviously either the preoptic area should be considered part of the hypothalamus (as some argue it should) or we should say that other areas beside the hypothalamus are involved in regulation against cold A question which currently looms large in our minds is Why does the anterior hypothalamectomized animal exhibit a near normal resistance to cold whereas the animal having the prechiasmal area removed exhibits such a striking deficit?

Lyman What if you destroy the hippocampus? What happens? Have you ever tried that?

Keller Not as a specific approach I don t recall whether we have tested animals having involvements of this structure in the cold room or not

Burton It should be mentioned with regard to your remark about other areas taking part that schizophrenic patients who have had leukotomies do show changes chronically in temperature regulation as regards their diurnal rhythm I think they also have some deficiency in their defense against cold We have to admit there are other areas don t we?

Keller When we first observed a deficit in such an animal before it was terminated we simply said tissue defect is considerably further caudal than we intended it to be but at necropsy it was found to be exactly where we thought it had been placed

The explanation of why observations on this type of preparation are not in the literature is that these animals have a very stormy postoperative course for several days They are wild and dynamic as described by Fulton and Ingraham (22) years ago But we have learned through experience how to tide some of these animals through the immediate postoperative period after which they quiet down and are subsequently reasonably easy to maintain This type of preparation can now be made with reasonable predictability

Burton Before we move away from Dr Keller s work I would like to say something which I think expresses the feeling of us all in case he doesn t know it We asked a lot of critical questions about his work but all of us have the most tremendous admiration for it and for his very great patience for 30 or was it 40 years We recog

nize the fact that no one else in the world has the surgical skill, or the patience, or the scientific discipline in histological verification that Dr Keller has We may be critical but we all envy him and wish we could do similar experiments

Horvath We all agree to that

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PHYSIOLOGICAL ASPECTS OF HIBERNATION IN MAMMALS

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It is necessary at the outset of my presentation to define hibernation Webster defines hibernation as passing the winter in a torpid or lethargic state If the phrase *true hibernation* is used for a lowering of body temperature this implies that any other type of hibernation is false Therefore we have coined the phrase *deep hibernation* to indicate the condition that I shall describe presently I define deep hibernation as a passive abandonment of the warm blooded state with body temperature reduced to a few degrees above the environment but with the animal retaining the ability to rewarm without heat from external sources With this definition in mind I shall confine myself entirely to rodents hamsters ground squirrels and woodchucks Since I have never worked with bats also deep hibernators according to the definition I will not discuss them I shall describe how rodents react in hibernation how they come out of hibernation and then how they go back in again

In the hibernation of rodents there are considerable differences that can be observed visually All rodents in hibernation are curled up in a ball In the case of the hamster the animal is immobile as far as one can see except for occasional respiration The cerebral cortex of the hamster is electrically silent when body temperatures are below 18°C The ground squirrels on the other hand in hibernation at about 5° to 7°C are not immobile They rock occasionally from side to side they shiver occasionally and they sometimes actually seem to stretch but then they go back into the ball like shape again

Henschel Does the body temperature go up during the movement?

Lyman A very slight amount

Horvath The reason for limiting your comments to these three animals is for convenience not because they are the only ones that are deep hibernators?

Lyman No there are a great many others These are the only ones I have worked with so I am on more familiar ground with them

Fremont Smith When you say that the cerebral cortex of the hamster is electrically silent you mean silent to what?

Lyman Silent in reference to outputs from bipolar electrodes with as high amplification as we could get

Keller Is it nonexcitable or spontaneous electrical activity which is absent?

Lyman It is silent unless stimulated in some way It is possible to obtain an evoked potential much lower down to 9°C in fact in the case of the hamster On the other hand as far as the woodchucks and the ground squirrels are concerned the cerebral cortex is not silent spontaneously down to at least 7°C and even at this low body temperature occasional small electrical excitations in the cortex are observed

Talbott *Might that be a function of the size of the animal?* Isn t the hamster considerably smaller than the ground squirrel?

Lyman The hamster is about half the size yes but I don t think size is the determining factor particularly Ground squirrels are true squirrels and hamsters are mice

As for vocalization in hibernation ground squirrels will give a sort of groan when picked up I have never heard them vocalize when undisturbed They can make a noise at 7°C Woodchucks give grunting sighs

Strumwasser (1) working with *Citellus beecheyi* which is a particular species of ground squirrel finds that while they are in hibernation and undisturbed they actually give a high pitched scream at 7°C He finds the cerebral cortex in this animal is active at all temperatures The hibernating animal retains many of his homeokinetic mechanisms For example the sensitivity to inspired carbon dioxide remains almost normal Figure 22 shows the number of *respirations per minute and the ambient CO to which the hamster* is exposed during the hibernating state When the per cent CO gets around 3 per cent an increase in respiratory rate results If the ambient CO is 9 to 10 per cent a great increase in respiratory rate is obtained If the animal continues to breathe this high CO for a long

time, it actually works so hard breathing it starts the waking process which I shall discuss presently

Taylor How do you measure the respiration?

Lyman By observing the breathing movements With very low rates (3 per 2 minutes), long periods of apnea and a Cheyne Stokes pattern this measurement becomes difficult The relationship of breathing rates to ambient CO_2 is illustrated in Figure 23 Actually the animal is nearly as sensitive to inspired CO at body temperature of 6°C as Davies Brow and Binger (2) found in human beings with temperatures of 37°C

Carlson That is rate of respiration in the hamster compared to volume of respiration in the human

Horvath That is right After all CO_2 does not stimulate respiration until after it attains a certain value whereas the respiratory rate changes the volume first and raises a question on Figure 23 because your rate did not increase a great deal until after you had gone past 5 to 7 per cent CO_2

Lyman It was lower than 5 per cent

Horvath The response of the respiratory system could have been

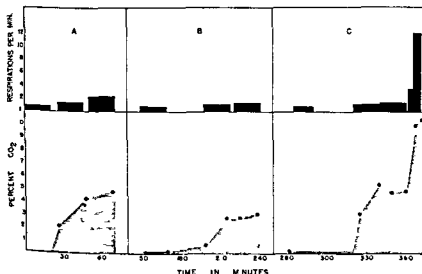


FIGURE 23 *Effect of inspired CO_2 on respiration in the hibernating hamster* Reprinted by permission from Lyman C P Effect of increased CO_2 on respiration and heart rate of hibernating hamsters and ground squirrels *Am J Physiol* 167 638 (1951)

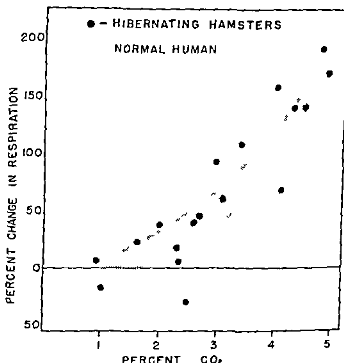


FIGURE 23 Comparison of effect of inspired CO_2 on per cent changes in respiratory volume in humans (2) and respiratory rate in hibernating hamster Reprinted by permission from Lyman C P Effect of increased CO_2 on respiration and heart rate of hibernating hamsters and ground squirrels *Am J Physiol*, **167**, *638 (1951)*

more adequately evaluated in terms of minute volume, but difficulties associated with the animal's sensitivity to arousal stimuli probably precluded such measurements

Lyman I think you will agree nevertheless, the animal is sensitive to remarkably low percentages of CO_2 Actually, the PCO_2 is lower than normal in the hibernating hamster

Bass How do you obtain samples for acid base determination?

Lyman Actually when the animal is in hibernation, this isn't much of a problem It can be done by cardiac puncture Where we had trouble was trying to establish a normal range for an animal awake I always was doubtful about a normal range We have a technique now by which we can intubate the aorta chronically and remove blood from time to time I shall talk about that later This would be the way to get the normal, but I don't think anesthetizing

the animal or restraining him in any way is going to give a true normal range.

The blood sugar of insectivore hibernators, such as the hedgehog, has been reported to be below normal values. Hibernating hamsters, ground squirrels, and Arctic ground squirrels have been reported as remaining in the normal range. Some confusion exists as to the pattern to be seen in the 13-lined ground squirrel.

As far as temperature control is concerned in hibernation, almost all rodents that I know about that hibernate passively follow the ambient temperature from about 5° up to about 15°C. If, however, the ambient temperature is lowered below 5°C, there is a thermoregulatory action which takes place, and the animal increases its oxygen consumption and maintains its body temperature at about 2° or 3°C. This is one of the reactions that can take place.

Another reaction which can take place, if the ambient temperature drops too rapidly, is that the animal is not able to compensate for the drop and therefore eventually freezes.

The third reaction that can take place is that the animal does not remain in hibernation with increased oxygen consumption but actually works so hard keeping warm that it starts the chain of the waking reaction.

Table I gives data from an experiment illustrating this in the hamster. The upper line shows that the chamber temperature was 5.8°C, and the cheek pouch temperature was 6.1°C. The hamster's cheek pouches are very useful, as they are enormous and thus facilitate the insertion of thermocouples for measuring body temperature.

In column 6, oxygen consumption is shown to be 60 cm³/kg/hr. If the ambient temperature is dropped to about 1° or 0.5°C, the cheek pouch temperature remains at 3.6°C, but the oxygen consumption rises about four, or almost five-fold.

As is shown in the table, this can continue for a considerable length of time; it lasted 24 hours in the case described, and then the measurement was terminated.

Line 4 gives the same idea, with the cheek pouch temperature remaining above ambient; in this case 2.2°C, which was the lowest we ever obtained in a hibernating animal.

Burch: When the oxygen increased four- or five-fold, did respiration change?

Lyman: I don't know. They were in the metabolism chamber. So I simply measured oxygen consumption.

Burch Was there more complete extraction of oxygen or more rapid ventilation?

Lyman I think there was more rapid ventilation

TABLE I

Effect of Near-Freezing Ambient Temperature on the Oxygen Consumption of the Hamster in Hibernation

Animal Number	Date	Duration of Measurement	Chamber Temperature (°C)	Cheek Pouch Temperature (°C)	O_2 Consumption (cm³/kg hr)
132	Feb 28	11 hr	5.8	6.4	60
	Mar 1 and 2	22 hr	1.0 to 0.5	5.6	273
131 A	Mar 16	8 hr	4.4	5.0	78
	Mar 16 to 17	21 hr	0.7	2.7 to 3.9 to 2.2	210
	Mar 17 to 18	15 hr	4.8 to 5.3	5.3 to 5.6	52
170	May 19	12 hr	5.5	5.5	32
	May 22 to 23	42 hr	1.7	4.4 to 2.7	187

Reprinted by permission from Lyman C P The oxygen consumption and temperature regulation of hibernating hamsters *J Exper Zool* 109 55 (1948)

Talbott Is there circulation of air by means of breathing through the cheek pouch?

Lyman No

Talbott That is an isolated compartment?

Lyman It is inside the mouth but it is outside the airway

Hock I should like to point out that in other hibernators the floor* temperature which causes this reaction may be lower For example, in bats I found roughly the same values at 5.8°C as Dr Lyman shows However, at 2°C there was a lower metabolic rate (3)

Lyman I suspect that if you do this with different genera, you are going to get different sensitive temperatures, but the point I am trying to make is that there is some temperature, and it probably is always above freezing where the animal actually has some homothermism, some ability to react against the cold, so it actually doesn't freeze In fact, I am quite sure that in the 13 lined ground squirrel,

*Editor's Note Colloquial expression probably referring to a "critical sensitizing" reference point (T°)

and probably in the Arctic ground squirrels this sensitivity would be found at a much lower temperature

Hock It is below 2°C and probably near 0° in the Arctic ground squirrel

Lyman In this regard we have worked with isolated hearts testing their ability to beat at low temperatures The isolated heart of

3°C The isolated heart of

−1°C My instrumentation

es below this level

Bass I didn t see the cheek pouch temperature I just saw the oxygen consumption On an oversimplified basis I would say that increased oxygen consumption possibly represents overcompensation Not being a biophysicist I would have predicted that the cheek pouch would have increased its temperature This seems a rather large increase in oxygen consumption percentage wise without an accompanying increase in cheek pouch temperature considering the relatively small change in ambient temperature

Lyman Yes it does

Bass Would some of the biophysicists comment?

Burton The difference between the cheek pouch temperature and the ambient temperature on February 28 is 0 6 degree The difference on March 1 and 2 (taking 0 5) is 3 1 which is just about five times as much difference of temperature Biophysicists would agree that five times the production of heat and heat loss corresponds to five times the gradient of temperature

Lyman Throughout the rodents as an order this same ability to regulate at low temperature apparently does exist although as far as I know the monitoring of the temperature and the measurement of oxygen consumption at the same time that the ambient temperature is lowered have not been done except in the hamster

The waking process normally takes place naturally in all hibernating animals from time to time The animal will remain in hibernation anywhere from 1 day to possibly 2 or 3 weeks and then it will wake from the hibernating state spontaneously If you stimulate the animal externally by poking it or pinching it for example the waking process will also take place This waking process is a very complicated and coordinated series of physiological events which are actually relatively easy to study because all that is necessary is a hibernating animal Observations can be made as it awakens

In order to wake some animals such as the 13 lined ground

squirrel considerable stimulation is necessary particularly if they have been in hibernation for a long time Almost anything will wake hibernating hamsters except noise as they apparently are deaf during the hibernating state But ground squirrels must be poked turned etc then the waking process starts

Keller Have any analytical observations been made on the deafness whether it is in the receptor or centrally?

Lyman Yes Kahana (4) worked on the auditory nerve of the hamster when Chatfield and I were working on the waking process He found that the auditory nerve of the hamster failed to conduct below about 18°C A spike potential could be elicited from the auditory area of the cortex when we shouted at a hibernating woodchuck but ringing a loud alarm for about an hour near a hamster failed to awaken the animal

Reynolds What about bright light?

Lyman It doesn t seem to bother them The animal is curled up his head is almost under his tail and the head is in shavings I have no indication that turning lights on or turning them off will wake an animal

Reynolds Some experiments were performed at Yale University a few years ago on the disappearance or the elevation of threshold with the onset of anoxia or different kinds of sensory stimuli It seems to me visual stimuli went out first and auditory next and then touch and other responses like that in that order I wondered if there were any gradations of this type involved here In other words it would be hard to test light I am sure when the animal had his head tucked under

Lyman They are quite sensitive to tactile stimuli

Reynolds Tactile would be the last you would expect to disappear in any kind of submergence

Lyman The peripheral nerve of the hamster will conduct down to about 3°C

Montgomery Could there be deep body response to handling?

Lyman There could be Turning hamsters will start arousal but sometimes you have to squeeze the 13 lined ground squirrels a bit to get them started

Siple Have you experimented with variations in radiation on the animals in comparison to changes in air temperature?

Lyman Dr Davis and I heated some animals with diathermy

Davis If I remember rightly they wake up with a higher tempera

ture than when they wake up normally We speeded up this rate of temperature increase to normal temperatures from the usual value of 4 hours to 1½ hours We found also that their heart rate did not increase as it does when they wake up normally with the rise in temperature but that there was a lag followed by a sudden acceleration

Lyman As far as waking them is concerned we stimulated them anyway because we picked them up and moved them to another room etc

Davis We tried to keep the two animals we worked on very quiet We were careful but how successful we were in our care is a question We did find if the waking up process had already started for instance the heart rate came up normally

Lyman You could warm them without increasing heart rate In the waking process the first physiological event that can be observed is an increase in heart rate This anticipates any rise in body temperature

Concurrent with the increase in heart rate or following slightly afterward there is an increase in muscular activity which can be picked up with the electromyograph

The top of Figure 24 is the record of an animal that had just been fitted with electrodes The heart rate is about one beat every 3 or 4 seconds which is of course fast The heart has already started to accelerate As the animal started to wake up temperature rose and the heart rate increased and finally muscle action potentials were seen So at number 5 on Figure 24 the muscle action potentials almost obliterate the heart rate It is evident that at the same time the heart is speeding

Hart At what point do you notice the increase in oxygen consumption? Does that increase simultaneously with the increase in heart rate?

Lyman I am not too satisfied with the oxygen consumption data at lower levels The way I did the experiments originally was to pick up the animals and put them in metabolism chambers The chamber had to equilibrate before I got good records So the oxygen consumption shown is not the true oxygen consumption at the start of the waking process What should be done is to have the animal go into hibernation *in the metabolism chamber and then* have it stimulated in some way and observe the oxygen consumption The number of respirations a minute starts to climb very rapidly

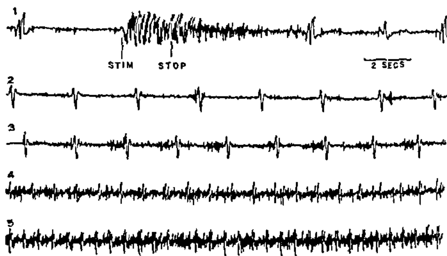


FIGURE 24 Electromyogram and electrocardiogram of hamster waking from hibernation Reprinted by permission from Lyman C P and Chatfield P O Mechanisms of arousal in the hibernating hamster *J Exper Zool* 114 491 (1950)

Hart As soon as the increase in heart rate?

Lyman I couldn t say exactly I suspect very soon afterward if not at the same time

Figure 25 shows that the rise in heart rate is very dramatic From a heart rate of 6 to 12 per minute, the rate will increase to 550 beats per minute within 3 hours This curve does not follow the Arrhenius equation It is simply a physiological curve

Talbott What is the normal heart rate?

Lyman About 300 to 350

Molnar You say that you artificially awakened all of these animals on which you studied the heart rate?

Lyman That is right

Molnar Have you measured the heart rate in the ones awakening naturally?

Lyman Not to form a graph like this, no

Molnar Is this acceleration of the heart a response to your stimulation rather than an incident occasioned by the awakening? Is the acceleration necessarily a primary event in natural awakening?

Lyman I think the speeding of the heart is one of the essential functions which start with the waking animal To turn to word

chucks for a moment when we have observed them in hibernation and coming out we have seen an increase in heart rate an increase in electrical activity in the muscle and then a rise in body temperature

Molnar Is this observed without artificial awakening?

Lyman The animal is doing it himself However this is in the woodchuck and not in the hamster This is harder to do in the hamster because so many wires are necessary on the hamster And the animal is small and isn t too happy about it

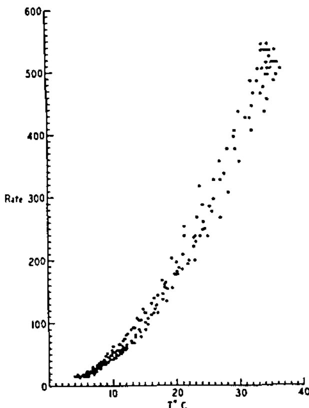


Figure Rise in heart rate in hamsters waking from hibernation Reprinted by permission from Chatfield P O and Lyman C P Circulatory changes during process of arousal in the hibernating hamster *Am J Physiol* 163 566 (*1950*)

Burton The electromyograph shows these rates?

Lyman In the initial electromyograph, you do not see shivering

Burton You could have "subclinical" shivering?

Lyman Yes In the case shown at the top of Figure 24, the animal was simply poked lightly with a pencil at the point where it says STIM A burst of muscle activity resulted This activity was invariable, when this was done

Burton You could see that?

Lyman On the electromyograph

Talbott Were these implanted electrodes or superficial skin electrodes?

Lyman They were skin electrodes

Talbott But not implanted?

Lyman They were sewed into the skin

Talbott Inserted through the skin into the muscle?

Lyman No they were in the skin

Burton That suggests there are electrical events without mechanical events in the muscle fiber I was wondering if that meant there were electrical events without mechanical events in the fibers that were responsible for that

Lyman That is what I thought and I still think Certainly, there is no gross clinical shivering such as Dr Keller was talking about

Burton What about the subclinical factors you couldn t see?

Fremont Smith It could be isometric

Burton There also could be active movement which you couldn t see

Lyman It may be visible with a microscope but it is not with a dissecting scope

Keller It might not be able to get from the nerve to the muscle It might be motor end plate

Lyman You may be right

Keller Yes, but that would be just a muscle, wouldn't it?

Lyman Yes

Carlson Was there shivering?

Lyman Yes, by the time line 5 of Figure 24 was recorded the animal was really shaking This is a record of heart and shivering but I think that Figure 26 may clarify the problem of warming without shivering The top line of Figure 26 is cheek pouch temperature and the bottom is rectal temperature of an eviscerated, curarized animal waking at 5°C The test of the curarization in this case was

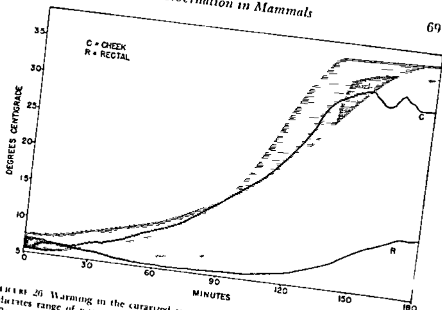


FIGURE 26 Warming in the curarized eviscerated hibernating hamster Shaded area indicates range of warming in cheek pouch and rectum of normal waking hibernators Reprinted by permission from Lyman C P and Chatfield P O Mechanisms of arousal in the hibernating hamster *J Exper Zool* 114 491 (1950)

stimulating the sciatic nerve and we recorded no muscle twitch The curare paralyzes the respiratory muscles before it paralyzes the skeletal muscle in this particular animal So, this animal was on artificial respiration We naturally did not expect him to be able to warm at all but nevertheless cheek temperature reached 33°C At the time, we were somewhat at a loss to understand it, and we still are We repeated this experiment several times

Fremont Smith There is more rapid rise in cheek pouch

Lyman I will discuss that, but first I wanted to show that the curarized and eviscerated animal still has enough sources of heat to warm

Figure 27 shows the animal waking from hibernation There is an increase in oxygen consumption, in heart rate and in muscular activity with a rather peculiar distribution of body temperature This graph shows the cheek pouch and rectal temperatures of the animal upon awakening from hibernation also the oxygen consumption is given The cheek pouch temperature rises quite rapidly At the end

of about 170 minutes the cheek pouch temperature has reached its normal homothermic level, about 37° to 38°C

Fremont Smith Were there any motions in the cheek pouch any muscular movements?

Lyman No

Fremont Smith Have you ever inserted a balloon to see?

Lyman No I haven t

Fremont Smith Certainly there is a lot of muscle there isn t there?

Lyman There is in the case of these animals where the cheek pouch thermocouple was back over the thorax The cheek pouch goes way back over the scapula So, the cheek pouch thermocouple was actually at the thoracic level Thus there was considerable activity of the muscles below the cheek pouch There are very thin muscles on the outside of the cheek pouch I doubt if they were contributing much

Fremont Smith Could you see any muscular movement at the thorax?

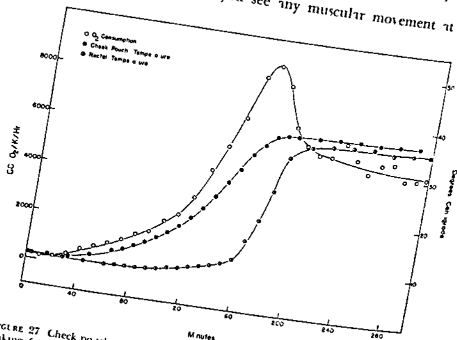


FIGURE 27 Cheek pouch and rectal temperatures and oxygen consumption of hamster waking from hibernation Reprinted by permission from Lyman C I The oxygen consumption and temperature regulation of hibernating hamsters *J Exper Zool* 109 55 (1948

Lyman Yes because the animal by this time had started the rapid temperature climb and the front legs were actually shaking the whole shoulder in the process

Fremont Smith So this is where your heat is coming from?

Lyman Yes On the other hand the hindlegs do not shiver Actually if an animal is taken out at say 130 minutes it can crawl with its front legs but it will drag its hindlegs This problem of distribution of heat caused us to look into the circulatory condition of the animal as he was waking from hibernation

Before I continue with Figure 28 I would like to point out that there is quite an overshoot of oxygen consumption Actually at about the time the posterior part of the animal warms the oxygen consumption reaches its maximum level which is about 2½ times that of the animal under normal conditions in the cold room

Figure 28 is a study of the distribution of circulation in the normal nembutalized animal Thorotrast was injected in the heart of the nembutalized animal The carotid the aorta the coeliac axis *and the femoral arteries are visible The time of injection took about* 1 second This picture was taken approximately 3 seconds after the injection



F re 8 X a of c r ula the n rr al a es l et zed ham er Rep r ed by pe sso f o L n a C P a d Cl atfe l l P O Mechan s of a o sal the l berna g l an e *J E pe Zo* 114 49 19

of about 170 minutes the cheek pouch temperature has reached its normal homothermic level, about 37° to 38°C

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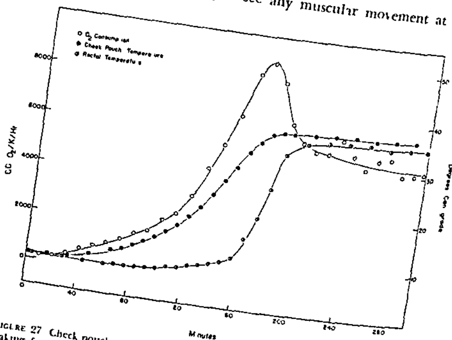


FIGURE 27 Cheek pouch and rectal temperatures and oxygen consumption of hamster waking from hibernation Reprinted by permission from Lyman C P The oxygen consumption and temperature regulation of hibernating hamsters *J Exper Zool* 109 55 (1948

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FIGURE 28 X ray of circulation in the normal anesthetized hamster Reprinted by permission from Lyman C P and Chatfield P O Mechanisms of arousal in the hibernating hamster *J Exp Zool* 114 491 (195)

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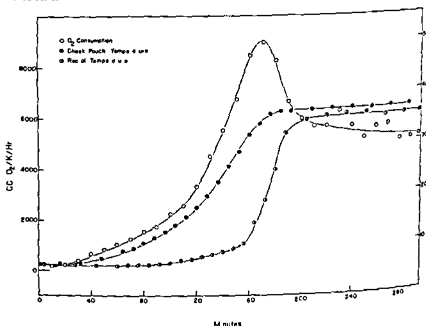


FIGURE 27 Cheek pouch and rectal temperatures and oxygen consumption of hamster waking from hibernation Reprinted by permission from Lyman C P The oxygen consumption and temperature regulation of hibernating hamsters *J Exper Zool* 109 55 (1948)

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FIGURE 28 X ray of circulation in the normal anesthetized hamster Reproduced by permission from Lyman C P and Chatfield P O Mechanisms of arousal in the hibernating hamster *J Exper Z* 114 491 *195*

A similar procedure was carried out in a hibernating animal 30 minutes after the start of arousal, and the distribution of the thorotrast 30 seconds after injection can be seen in Figure 29 Actually, with other x rays, it is possible to see that the thorotrast goes up to the brain and returns, but, as is evident from the outline of the aorta thorotrast has not yet reached much beyond the level of the kidneys So, we postulated there is a differential vasoconstriction in the posterior part of the animal and that, during the waking process, the front part warms because of this vasoconstriction in the posterior part Then, when the posterior portion is warmed, the vasoconstriction ceases, and the temperature of the posterior portion rapidly rises, as was shown in Figure 27

Burch I have seen this phenomenon before If vasoconstriction persists for a long period of time, which this would suggest, wouldn t the smooth muscle require some blood to keep itself alive and to remain constricted?

Lyman Yes

Burch What is the source of blood supply?

Lyman The vasoconstriction I am sure is not complete

Burch Is it intermittent and a sort of hunting phenomenon?

Lyman I don t think it is hunting I think there is some circulation at all times For instance, if the hind toe is cut off, blood will



FIGURE 29 X ray of circulation in the waking hibernator Reprinted by permission from Lyman C P and Chatfield P O Mechanisms of arousal in the hibernating hamster *J Exper Zool* **114** *491 (1950)*

seep from it very slowly But if the front toe is cut off there is a much more rapid flow though I haven t done any measurements

Keller I believe the oxygen consumption of smooth muscle is considerably less than striated muscle is it not?

Lyman I don t know

Burton Is there any hint of an anatomical stricture in the aorta?

Lyman As far as I know there is not I made one complete series of sections from the posterior portion of the aorta right forward anteriorly as far as it went There was no indication of constriction or muscular band or anything like that which would cause a clamp ing off at a certain level I think it is peripheral vasoconstriction and not a more central one

Behnke Dr Irving isn t this the same type of cut off that sea mammals have?

Irving From what Dr Lyman said it isn t quite the same be cause in diving vasoconstriction apparently all of the musculature is simultaneously involved and the circulation generally reduced Here during the waking reaction at any rate the distribution of the peripheral circulation is differentiated as between the front and the hindlimbs

Lyman Yes between the anterior portion and the posterior por tion I am a little bit unhappy about the front limb because it is pale during the waking process whereas it is normally pink I think there may be some vasoconstriction at the foot but it is open above the foot

Irving Of course the recovery of function in the forelimb ahead of the hindlimb is suggestive likewise of a difference isn t it?

Lyman There is certainly some

Hock During arousal in the Arctic ground squirrel I have found deep colonic temperature to be 35° to 37°C while the temperature of the hindfoot is still 2 5°C In fact the hindfoot is not yet operat ing when the colon is at normal active temperature but is at about the same level at which the colon was at the beginning of arousal

Lyman Figure 30 presents a series of temperatures These tem peratures during the warming process show that the anterior portion in general warms early while the posterior portion remains cool until some 90 to 120 minutes later when it starts to rise

Davis The temperature of the cerebrum is below that of the heart and cheek pouch?

Lyman Yes The heart is the warmest place during the waking process

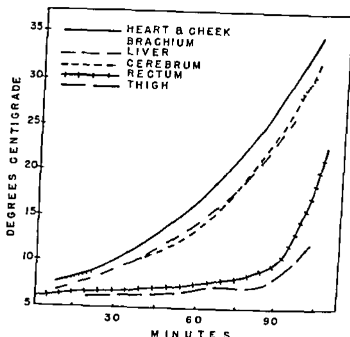


FIGURE 30 Temperature of various areas as animal wakes from hibernation Reprinted by permission from Lyman C P and Chatfield P O Mechanisms of arousal in the hibernating hamster *J Exper Zool* 114 *491 (1950)*

Davis Is this because it is working the hardest?

Lyman I think so Actually the heart is pumping against a high peripheral resistance and pumping at a very rapid rate So it is actually a very inefficient pump and is working therefore as a very effective heat generating organ

When viewing Figure 26 which shows the eviscerated curarized animal warming we suggested that perhaps all the energy of warming was coming from the rapidly beating heart Some individuals have calculated the heat output and have indicated that speeding the excessively rapidly beating heart could not possibly produce the quantity of heat required to induce this animal to become warm

Keller What possible sources of heat would the eviscerated animal have other than heart?

Hart Skeletal muscle

Keller But the nonshivering muscle cannot produce adequate quantities of heat

Lyman There is no early shivering With the waking process there is a rapid increase in blood pressure as is suggested in Figure

31 Furthermore there is an atrioventricular dissociation which sometimes is seen in a number of hibernators A of Figure 31 indicates that the P wave is completely dissociated from the QRS complex About 30 seconds later the heart rate is now completely organized as is shown in B of Figure 31 Within a short period of time after initiation of the warming process there occurs a rise in both blood pressure and organization of the heart

Burton Does the sudden opening of the peripheral circulation to the hindlimbs agree with the reaching of a certain blood pressure or is the timing not in favor of that hypothesis?

Lyman We don't know that at the present time When we made these records we had great difficulty in recording blood pressure We were recording from the carotid artery and the carotid artery

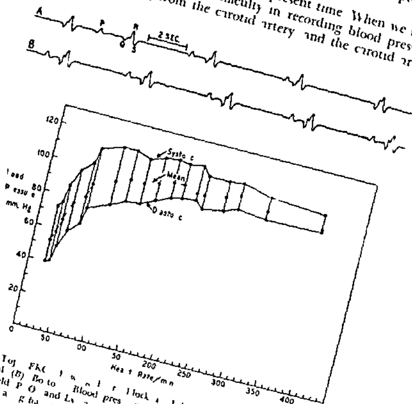


Figure 31 Top: EKG [illegible] heart block [illegible] hibernator (A) [illegible] appearance [illegible] arousal (B) Bottom: Blood pressure during arousal Reproduced by permission from Chatfield P O and Lyman C P Circulatory changes during process of arousal in the hibernating hamster Am J Physiol 163 566 (1950)

in the hamster is made out of something like a high grade of tissue paper As soon as the animal really started to shake and shiver the artery tore and the experiment was terminated *Since that time we have been able with the intubating technique to record blood pressures throughout the entire waking process*

Another thing which occurs during the waking process is a glycogenolysis mobilization of the glycogen in the muscle and in the liver It is possible to trace this using either microchemical techniques or the periodic acid Schiff reagent which is specific for glycogen and taking small samples of liver during the waking process

Three sections taken from the liver of an animal during the waking process are shown in Figure 32 The left one indicates that when the animal was first rousing the liver was loaded with glycogen in about the middle of the waking process when the anterior portion of the body was about 20° or 15°C the liver was more depleted and finally at 35°C the liver glycogen was further depleted

Fremont Smith At what time interval?

Lyman The first sample was taken approximately 15 minutes

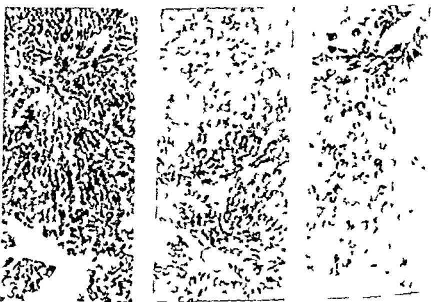


FIGURE 32 Diminution of liver glycogen during waking from hibernation Reproduced by permission from Lyman C P and Leduc E H Changes in blood sugar and tissue glycogen in the hamster during arousal from hibernation *J Cell Comp Phys* 41 471 (1953)

the hibernating heart was about twice as high as in the normal heart, and it was depleted during the waking process Obviously, we did not take biopsies of the heart as we could with the liver during the waking process, but we did take a sample of the heart in hibernation and a sample after waking

Behnke Is glycogen formed in the liver during hibernation?

Lyman If this occurs, I believe it would be minimal

Burton Over a long enough period?

Lyman Yes Some of the older workers claimed they found a difference of 20 or 30 per cent decrease over long periods This was of course, with different animals, and the statistics aren't very good You obviously can't obtain a sample when the animal first starts to hibernate because he will arouse

Taylor Will diabetic animals hibernate?

Lyman I don't know, I doubt it I think they would die

Behnke What is the source of the animal s energy during hibernation?

Lyman The R Q is 0 7, so presumably they are using fat

Behnke Glycogen is just being held?

Lyman Yes

Hock This storage of glycogen in the heart and other parts of the hibernating body is perhaps one of the most significant differences between hibernators and nonhibernators The inability of the homotherm to store large amounts of glycogen, especially in the cardiac muscle, or more important, to mobilize it properly within a few hours like the hibernators, may explain why we cannot obtain a long term poikilothermic homotherm

Reynolds Did you do liver fats by any chance?

Lyman No

Reynolds I should think this would be worth doing, since at least in the case of the newborn there are large quantities of fat in the liver at birth which tend to disappear very slowly over a period of several days in contrast to the glycogen which disappears in the first 12 hours after birth It seems to me there might be some kind of parallelism in this process to the case of the hibernator

Hock The liver in the hibernator is extremely fatty

Reynolds When it goes into hibernation?

Hock Then and during hibernation It may be almost fatty degeneration

Lyman It varies In some it is more than in others

Keller Does a lean animal go into hibernation?

Lyman Yes it does

Keller On Dr Taylor's question about diabetes alloxan diabetes would work quite well and it would be very interesting

Fremont-Smith To see whether they would hibernate?

Keller Yes

Burton Similarly I would look at the lipid in the heart muscle which gives the stores of energy against fatigue etc rather than the glycogen It might be worth while at some future time to try to measure the lipid in the heart muscle and how it is depleted during the waking process

Fremont Smith If we are thinking of hibernation as returning to the fetal state it would be interesting to know whether the younger animals hibernate Have we any evidence?

Lyman Yes I have done an experiment with hamsters putting them into the cold room as soon as weaned and carrying them along Hibernation is not possible until the animals are approximately 100 days of age The very young animals would not hibernate I don't think there is any real relationship between the fetal state and the hibernating state The fetal state is not organized whereas the hibernating state is highly organized with this waking process

Hock Hamsters do not deposit fat on their bodies as do ground squirrels they store food Ground squirrels are born about 1 June in Alaska and emerge from the dens in July weighing 150 to 200 gm About 1 October or at an age of about 120 days they weigh about 500 gm and hibernate

Burch Have you labeled glycogen to see if there was any sequestration?

Lyman No

Burch Is there any relationship between the peculiar metabolic state of using fats in preference to glycogen? Is that also true of the migrating birds that similarly store food in preparation for the migration? Do they use fat in preference to the liver glycogen?

Hock I don't think anyone knows about glycogen storage and use during migration Birds store fat just before migration and use it then much as hibernators store and use fat But they appear to arrive at the northern terminus of migration in good condition

Irving Yes They don't exhaust the fat supply at least during migration to the Arctic because many species of birds arriving there are about as fat as they could be (5)

Lyman How about the golden plover is it fat?

Irving Usually it is fat

Burch. Is glycogen stored?

Irving. I don't know. My estimate of fat is simply by visual observation, which is quite reliable in birds because there are certain tracts in which the fat is clearly visible.

Behnke. Is the ability to store glycogen in the liver one of the chief differences between the hibernating and nonhibernating animal? Certainly the hamster or the rat that has fasted for 24 hours loses glycogen.

Hock. I think this is one of the most essential differences.

Keller. The core temperature difference there would be a factor. The hibernator's temperature is much lower, and that is probably one of the reasons for the slower disappearance of glycogen.

Hock. When he starts to mobilize it, he is still at low temperature.

Keller. The mobilization factor during spontaneous warming is superimposed on the maintenance factor in hibernation.

Behnke. There is some energy required at low temperature. Why isn't that energy derived from the glycogen rather than from body fat?

Keller. My point is that in hibernation, basal metabolic rate, tissue respiration, is at a low level.

Behnke. Is ... fied

fatty acid. In ... pro-

gresses or doe...

Lyman. I ... tain

change overs of fat in hamsters.

Irving. Is there any indication from the respiratory quotients upon awakening from hibernation?

Lyman. The respiratory quotient goes up during the waking process. This I haven't measured personally, but it is quite well established that such is the case.

Montgomery. Does withdrawing food a day or two, when the animal is ready to hibernate, throw him into hibernation more rapidly?

Lyman. This is true to some extent in woodchucks and ground squirrels, which store large quantities of fat.

Montgomery. I suppose the environmental temperature is still a big variable in inducing hibernation.

Lyman. Yes. Hamsters just simply starve. They do not protect themselves by going into hibernation.

Bartholomew (6) has shown recently that if *Perognathus longimembris*, the pocket mouse, is starved or deprived of water even

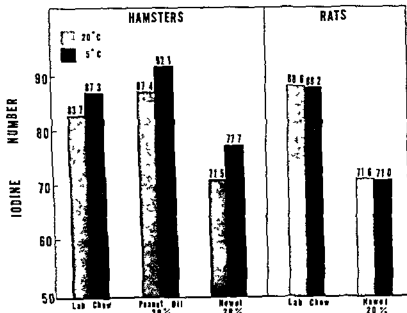


FIGURE 33 Effect of cold exposure on depot fat of hamsters Newol is a saturated cooking fat Peanut oil is unsaturated In every case cold exposure increased the iodine number and lowered the melting point of the depot fat in hamsters Reprinted by permission from Fawcett D W and Lyman C P The effect of low environmental temperature on the composition of depot fat in relation to hibernation *J Physiol* 126 *235 (1954)*

at 70°F, it goes into a lethargic state So these animals are apparently using this as a mechanism to protect themselves against starvation

Montgomery Are there any generalities about how soon over eating occurs at the termination of hibernation or does that vary considerably?

Lyman Ground squirrels and woodchucks grow fat very rapidly after the mating season Generally, when they come out of hibernation they lose weight for a period and then after the breeding season the weight gain is very rapid

Fremont Smith *Perognathus longimembris* is very interesting, because it suggests that there is a mechanism similar to hibernation which is not dependent upon temperature

Lyman That is right

Fremont Smith On the other hand, if that is a different mecha

technique I don't think they could be fed through the stomach tube because it would be too inconvenient

Taylor The livers were also removed from your eviscerated animals I assume?

Lyman The blood supply to the liver was completely cut off

Taylor So the liver glycogen wasn't playing much of a role in the energy processes in rewarming in those animals

Lyman In this case we took the liver sample at the start of waking and at the end of waking and the liver glycogen remained the same Blood sugar in the normal waking animal stays at 100 or it may sky rocket up to about 300 to 350 mg per cent In the case of the eviscerated animal the blood sugar was at a hypoglycemic level The animal's temperature attained nearly 37°C and then it died

Taylor Was the blood supply to the pancreas also interfered with at the same time?

Lyman The pancreas wasn't there

Taylor What about the adrenals?

Lyman The adrenals were in

Pace Did you see any evidence of ketosis?

Lyman No As far as I know it has never been reported

Behnke With reference to carbohydrate metabolism it has been reported (7 8) that in hypothermic individuals glucose administered intravenously in 5 per cent solution is not utilized *

Keller What is the depth of hypothermia?

Behnke In human beings it would be about 30°C

Lyman In the waking process there is peripheral vasoconstriction

[illegible]

waking process as far as the central nervous system is concerned to the heat regulatory centers

Dr Chatfield and I (9) did experiments with electrodes placed in various areas of the brain during the waking process and recorded from these areas during arousal We found no activity in the hypothalamic area but the electrical activity was confined to

*Editor's Note [illegible] Captain Behnke would like to add the following afterthought to his remarks at the conference [illegible]

[illegible] the isolated rat liver preparation cooling from 38° to 3[illegible]°C results in a decrease [illegible] blood glucose levels by as much as [illegible]0 mg/100 ml (e.g. from 160 to 110 mg/100 ml) Simultaneously there is an increase [illegible] liver glycogen [illegible] 0.3 to 0.4 gm/1[illegible] gm [illegible] tissue the extra liver glycogen deposited under these conditions almost quant[illegible] accounts for the decrease per [illegible] glucose These changes are promptly re[illegible] on rewarming such preparations to 38°C

nism, then it seems to me the other discussion on hibernation suggested we are possibly dealing with rate determinants in which there might be differential temperature effects on enzyme systems as opposed to fat and glycogen I wonder whether anything has been done This could be done *in vitro*

Horvath The humming bird shows exactly this If the supply of food is not available, his temperature drops It is related to the glycogen carbohydrate stores

Burton Dr Fremont Smith has raised a point which I was going to raise There may be a fundamental physiochemical relationship here In plant physiology, there is a great deal of evidence The over all respiratory quotient depends upon the temperature and becomes lower as the tissues cool The lower the temperature, the more trans formation from fat and carbohydrate The equilibrium moves the direction of fat going to carbohydrate rather than carbohydr to fat

In the warm blooded mammal, it is very easy to show carb hydrate going to fat, but it has been extremely difficult to show going the other way I suspect there is an underlying physiochemic shift in this process

Fremont Smith On the enzyme basis, probably

Burch You must do turnover studies The glycogen in the live may be turning over very rapidly and the size of the stores is bein maintained

Fremont Smith The suggestion was made that a sudden increas in heart rate might be initiated because of changes in glycogen metabolism within the heart at a very low temperature If this takes place, it would require an enzyme which converts glycogen at that temperature It seems to me this is entirely a testable hypothesis If it is so, it would be an extremely interesting thing to know, and it could be eliminated if the enzyme doesn t function

Behnke To account for the sudden increase

Fremont Smith Yes This is a very extraordinary phenomenon And apparently it is a primary one in the reawakening process

Behnke Have you fed any animals by means of a small tube, say continuously during hibernation?

Lyman I don t think that would be possible it might be

Behnke Certainly we can feed human beings continuously for periods of 24 hours during which time they are able to sleep with out a great deal of inconvenience

Lyman They could be fed intra arterially with this intubation

Lyman Under stimulus Dr Strumwasser (10) at the National Institutes of Health gets shivering in *Citellus leecheyi* (ground squirrel) at 6° and 7°C and the shivering occurs synchronously with discharges from the motor cortex rather than any other place in the brain Certainly this is not usually considered the heat regulatory center

Keller Failure to record electrical activity in the hypothalamus doesn't necessarily mean it is not active does it?

Lyman No I don't really think it does This depends considerably on the recording mechanism We did a large series of animals and I would be happy to find it in the preoptic area

Keller Also the electrical activity you pick up might be coincidental with the waking rather than the cause of the waking

Lyman It might be

Fremont Smith It might be a result of the awakening

Lyman Figure 35 was made quite a few years ago It shows a hamster entering the hibernating state At zero the lines of oxygen consumption and cheek pouch temperature are shown as gentle curves This was because the measurements were so erratic that we were not able to determine which started to drop first the oxygen consumption or the cheek pouch temperature

Later we were able to make some measurements on woodchucks

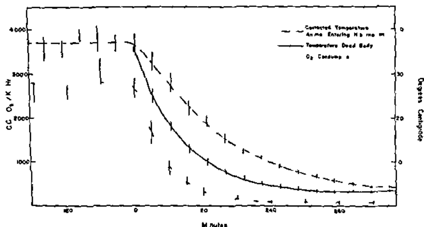


FIGURE 35 Record of hamster entering hibernation Reprinted by permission from Lyman C P The oxygen consumption and temperature regulation of hibernating hamsters *J Exper Zool* 109 55 (1948)

the so called limbic system that is, the cingulum, hippocampus fornix, and mammillary bodies

Fremont Smith Did you get into reticular formations?

Lyman Yes, and found no activity except at high temperature

Figure 34 illustrates the type of activity we observed in the hippocampal area are at rather high temperatures with increase in spike discharge as the temperature rose

Fremont Smith This is the waking animal?

Lyman Yes

Fremont Smith Spontaneously awakening?

Lyman Yes

Fremont Smith Under stimulus?

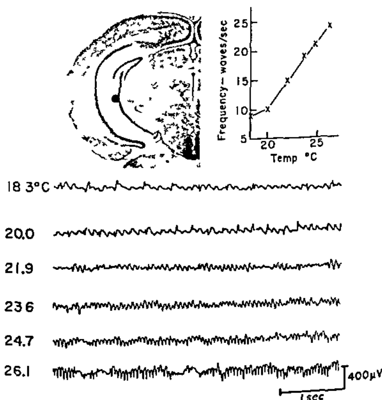


FIGURE 34 Electrical recording from area of black dot (hippocampus) of hamster waking from hibernation Reprinted by permission from Chatfield P O and Lyman C P Subcortical electrical activity in the golden hamster during arousal from hibernation *EEG Clin Neurophysiol* 6 403 (1954)

Lyman Under stimulus Dr Strumwasser (10) at the National Institutes of Health gets shivering in *Citellus leecheyi* (ground squirrel) at 6° and 7°C and the shivering occurs synchronously with discharges from the motor cortex rather than any other place in the brain Certainly this is not usually considered the heat regulatory center

Keller Failure to record electrical activity in the hypothalamus doesn t necessarily mean it is not active does it?

Lyman No I don t really think it does This depends considerably on the recording mechanism We did a large series of animals and I would be happy to find it in the preoptic area

Keller Also the electrical activity you pick up might be coincidental with the waking rather than the cause of the waking

Lyman It might be

Fremont Smith It might be a result of the awakening

Lyman Figure 35 was made quite a few years ago It shows a hamster entering the hibernating state At zero the lines of oxygen consumption and cheek pouch temperature are shown as gentle curves This was because the measurements were so erratic that we were not able to determine which started to drop first the oxygen consumption or the cheek pouch temperature

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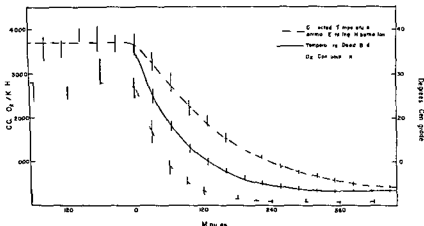


FIGURE 35 Record of hamster entering hibernation Reproduced by permission from Lyman C P The oxygen consumption and temperature regulation of hibernating hamsters *J Exper Zool* 109 55 (1948)

As they are bigger, it is possible to get more apparatus on them With electrodes, thermocouples, and oxygen consumption (Figure 36), we obtained some fairly good records from animals entering the hibernating state In the case of the woodchuck, when the animal is going into hibernation, the body temperature does not generally follow as a smooth curve, but the animal periodically shivers his heart rate increases, and the oxygen consumption rises Then the woodchuck stops shivering and again slides down toward the hibernating state The left hand part of Figure 36 shows where the oxygen consumption, heart rate, and body temperature rise, start to drop rise once again, and then start to drop

The interesting thing to me is that in every case the heart rate and the oxygen consumption anticipate the drop in body temperature This looks to me as if the animal is actively cutting off some sort of regulatory mechanism

Burch You mean when entering hibernation, the heart rate slows and when coming out, the heart rate increases?

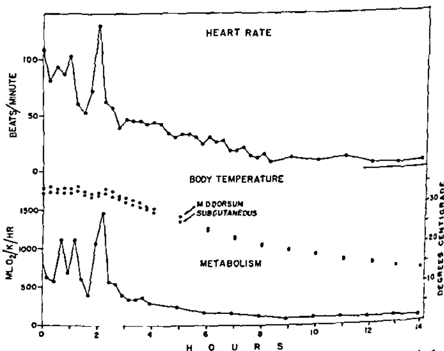


FIGURE 36 Record of woodchuck entering hibernation Reprinted by permission from Lyman C P Oxygen consumption body temperature and heart rate of woodchucks entering hibernation *Am J Physiol* **194** 83 (1958)

Lyman Yes

Fremont Smith He sort of alternates between going in and coming out He is really doing both of them momentarily

Lyman Yes

Fremont Smith This is analogous to subjects given typhoid vaccine The temperature rises to about the highest point and vasodilation begins suddenly in the periphery The next moment they feel chilly and cold and there will be vasoconstriction and another shift begins so that in 15 minutes they may be back in complete vasodilation They alternate between constriction and vasodilation

Lyman This is true in the woodchuck and the ground squirrel but as far as I can see in the hamster this does not occur There is a smooth curve Once he pulls on the brake or whatever he is doing down he goes We do not have the measurements on the hamster yet but we hope to have them

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LIVING ON THE SOUTH POLAR ICE CAP

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FOR PURPOSES OF BACKGROUND I shall begin with a brief description of living conditions at the Amundsen Scott IGY South Pole Station The station is located at the geographic South Pole close to the center of the Antarctic Continent All of the supplies for that station had to be sent through McMurdo Sound more or less in a direct line from New Zealand McMurdo is as close as supply ships could get to the continent and the most suitable locality for an airstrip on sea ice for US Air Force C 124 Globemaster cargo carriers and the Navy ski planes that airlifted personnel into the Pole Station

Until the Pole Station was established supplying an inland station entirely by air was virtually impossible I use the word impossible advisedly in the sense that up to this time our technology had not advanced to the point that it was conceivable for such an amount of tonnage to be flown in The use of Navy and Air Force equipment that had been developed shortly after the war made it possible to fly in and drop the supplies

Personnel and some of the delicate scientific equipment went in on ski planes The elevation at the Pole Station was around 9200 feet However as this was established by barometer it could be in error by several hundred feet in either direction

During the summer period when there was daylight for approximately 6 months the warmest temperatures were just slightly above 0°F * so the combination of temperatures from −20° to 0°F and the relative calm which characterized the summer were not at all unpleasant The greatest difficulty in the summertime was reduction of oxygen at that elevation

*EDITOR'S NOTE These temperatures were not changed to degrees Centigrade as most people are accustomed to ambient temperatures being recorded as degrees Fahrenheit The lowest ambient temperature of −100°F is equivalent to −73 3 C

In the wintertime, during the 6 months of darkness the minimum temperature averaged −73°F One month averaged −80°F The absolute minimum was −102°F We experienced as much as 97 consecutive hours when the temperature remained below −90°F As I recall, there were 149 days when the temperature remained below −40°F

In most areas of extreme cold, there is also a great deal of calm This is not so at the South Pole On the contrary, the wind blew incessantly all winter long at an average of about 15 knots, or around 17 miles per hour, with a maximum velocity of about 47 knots or 50 miles per hour There was not a single hour of calm recorded outside Therefore, the combination of wind and temperature or the wind chill factor, was extremely high

Our living conditions were superior to those of any previous expedition with which I had been associated For example, at the South Pole Station with a complement of eighteen men the heated area was four times the size of the station of Little America I, where 30 years ago forty two men lived This meant that we had something around ten times the floor space On the first expedition, I lived with seven other men in a room that was 10 by 10 feet square However at the pole, we had almost that much space per man and were able to give the men a choice of individual cubicles in which to sleep and keep their personal gear Actually, ten of the men paired off and in each case used the extra room as a little sitting room

Our galley, radio room, and meteorological facilities were in one building Another building was a barracks over 52 feet long which housed twelve men The other six men lived in a Jamesway hut which looks like a Quonset hut but is made of fabric, with glass wool insulation and a wood frame 8 feet high in the center and about 16 feet across We used diesel oil space heaters and electric fans to keep the warm air circulating A 30 kw generator was put on the line all the time so there was very little trouble with electrical units

There was nothing unusual about our food except that there were no fresh items other than a few fresh frozen items for special occasions

Burch What was the temperature inside the rooms?

Siple The inside temperatures ranged normally from 55° to 65°F, which was comfortable because we wore cotton or cotton wool underwear, with wool shirts and trousers It never got cold enough to make our fingers stiff We were able to sleep in our barracks building with only sheets and blankets in contrast to the bulky fur or feather sleeping bags of the past

Blair How much outside air did you provide?

Siple There was no provision specifically for the entrance of air except through the cracks although we made some openings in the mess hall near the stove The floor of the Jamesway was well below freezing

The whole camp was incorporated inside a framework of chicken wire and burlap with necessary lumber to hold up the passageways You could walk between the buildings without having to put on additional clothing despite the fact that temperatures in the passages dropped down in the middle of winter to around −60° or −70°F and in our coldest period it probably dropped to −80°F for a few hours at least

Burton How did you measure the −80°F? What sort of thermometer did you use?

Siple We had two types We had both the thermistors and the toluene thermometers The Weather Bureau provided us with thermometers capable of registering as low as −130°F Our electrical resistance thermometers could have registered even lower than this

I will explain later the clothing we used at different times We had primarily Army Quartermaster clothing which for the civilian scientific group was supplemented with some down filled clothing The Navy personnel intermixed Navy and Army clothing

I was at the pole for exactly 1 year Some of the personnel were there from 8 to 10 months We had one animal a dog that ran around outside at −90° to −100°F I don t understand this because we have had dogs in the past whose paws were frozen at higher temperatures

With this background I will start the second phase of my discussion At this point however I would like first to read a set of conclusions on the living conditions The reason for stating my conclusions at the beginning is to permit you to consider their validity as I proceed

First wind chill is lessened by altitude because air at the altitude of the pole was less dense and therefore had a lower thermal capacity than sea level air

Burch What is the altitude at the pole?

Siple It is 9200 feet plus or minus 100 feet The second conclusion *is that at least half a day of exposure* is *possible* under virtually any natural extreme of cold environment We were able to be outdoors as much as 4 hours moving around in conditions as severe as are likely to be found in nature

Fremont Smith How much acclimation does a man need before such exposure is possible? Is half a day of exposure possible immediately upon arrival?

Siple No In fact from my experience men do not become acclimated for at least 2 weeks

Fremont Smith How much exposure could a person fresh from a temperate climate endure 10 minutes or 2 minutes?

Siple Probably half an hour or so under extreme conditions before becoming rather miserable Altitude was a first consideration It masked the effect of other factors The men were inclined to be uncomfortable and not too efficient for the first couple of weeks

Keller What kind of work were they doing?

Siple Heavy physical labor They have to keep moving

Keller There must be enough exercise in order to raise body temperature?

Siple Yes temporarily but they are not able to maintain it for long Working hard at −60°F day after day I was not able to maintain comfortable body temperatures for more than an hour at a time

Fremont Smith When people first arrive for how short a time would you insist that they expose themselves at first before coming back into the warmth again 10 minutes or half an hour?

Siple We put some of these men to work the first few days for two periods of 2 hour stretches at −40°F

Fremont Smith Then 2 hours right away is possible?

Siple Yes at −40°F

Taylor Admiral Dufek and his party landed at the pole for the first time at the end of October 1956 The group had come from New Zealand between the 16th and 20th of October and then on about 31 October they flew to the pole These were unacclimated people The temperature was about −55°F They were on deck for just a few minutes but they experienced frostbite and severe discomfort from the combination of altitude and cold Certainly −55°F was quite warm compared to their later experience but these were probably the most unacclimated people who had ever been to the pole Don t you agree?

Siple Actually none of them was an experienced cold weather man Most of them had never experienced such low temperatures before the plane crew particularly They were literally frightened by the cold

There was one additional factor which increased the stress of the natural environment—the blast of the propellers around the plane which added considerably to their discomfort They were also

apprehensive because they weren t certain that they were going to get the plane off the surface In fact they almost didn t they had a great deal of trouble This was a very good test case for as Dr Taylor mentioned these were completely unacclimated people who had been dropped for a few minutes into almost lethal cold sur roundings

However under somewhat similar temperatures other groups of unacclimated men were able to work outside for a couple of hours at a time They knew however that there was a comfortable warm place available

These men performed very useful work We built a fairly large barracks building in a period of 2 working days The achievement was remarkable because of the high altitude and lack of acclimation of the new men They had to carry very heavy building panels

A result of such heavy labor and extensive exposure under polar conditions was a tendency to loss of body weight deterioration of strength and joint aches

I will read extracts of the notes I made in the Antarctic at the time I realize some of the conclusions now are not the same as those I wrote down while I was still cold but I thought these original notes might be of more significance I will give in somewhat chrono logical order the events that seemed to me to be important as I went along from day to day

I would like to point out that in these notes there are a few per sonal ailments mentioned One was apparently an allergy of some sort that caused nasal congestion which seemed to be aggravated by cold Throughout this early period I was attempting to refrain from using nose drops and antihistamine compounds but the aggra vation of the cold air necessitated their continued use I thought this might be of some significance as a background

Blair How much of that do you think was caused by dryness of the quarters in which you were living and drying of the nasal mucosa rather than the actual cold itself?

Siple When I stayed in the quarters which were very dry even though we tried to raise the humidity the condition improved Dr Howard Taylor who was our medical officer at the pole and who had been at McMurdo the year before was the first to suggest that this was possibly an allergy which may have developed over the long number of years I have been exposed off and on to cold Whether this is a misleading suggestion or not I leave to you to surmise from some of the evidence I have given

Talbott Did you wear a nose guard or nose mask?

Siple I did not At low altitude I had worn one by preference, when temperatures were −40°F. or lower, but at the pole I preferred whiskers to the mask, simply because I felt I wasn't getting enough air when I wore the latter. Anything that restricted the feeling of getting enough ventilation was disliked by most of the men Only two or three men consistently wore face masks when they went out The rest preferred to take a chance of getting their faces nipped or wore their hoods for protection

Burch Did you place thermistors in the nasal passages?

Siple No

Burton On the matter of allergy, I wonder if hay fever can occur in this climate There is a lot of evidence in the old medical books that people who are hay fever victims suffer an aggravation in the cold

Siple Maybe I have hay fever I do not know Whatever the cause of the allergy, at least there was an aggravation because of cold air

Burton Do you suffer from hay fever in the ragweed season each summer?

Siple I have not particularly noticed that I suffer the way other people do In my case, I have noticed a seasonal change, it seems to be a little worse in the winter than it is in the summer

Burton I don't know where the hay would come from in the Antarctic Some other agent may be the cause

Siple I want to make it very clear that I do not know the cause, but the condition existed and seemed to be aggravated

Horvath You had no other signs besides the nasal congestion? There was no urticaria?

Siple One other sign was a condition of psoriasis of the skin and particularly the nails It is rather interesting that the nails became almost completely cured, better than they had been for 2 or 3 years previously Then the nails deteriorated again

Fremont Smith While you were there?

Siple While I was there During the first half year when I was exposed to the greatest cold, there was an improvement in the nails However my nails were not the only ones affected One of the things that became apparent, as we examined one another's nails out of curiosity, was a reddened line, about an eighth of an inch back of the quick line My own was a very reddened line which persisted during the whole period while my nails were improving We found the same reddened lines on the nails of many of the men who were especially exposed to the cold In addition, there were also many longitudinal dark lines on the nails I had observed similar black

lines once before after my nails had been treated by x ray In both cases the nails felt a sort of tightening and tenderness while the black lines persisted

Burch Did you have anyone check to see if you had pernio or cold allergy? Cold allergic reactions of the skin may be mistaken for psoriasis

Siple Actually the doctor who was treating me for psoriasis had at the time of first acquaintance discovered the work of the Macy Cold Injury Conferences as well as other cold weather studies and was following up a theory that some adult skin ailments were actually due in some cases to cold injury suffered earlier in life So at least the doctor was aware of the potentiality Although such an assumption is not necessarily the reason for my own it was not completely ruled out

Actually the first time I ever became aware of this condition particularly the manifestation of it in my scalp I thought it was due to some tropical disease It appeared during the period I was at Indian B e living out under simula on my face parts of my l of the disease on my face and most of my body but the scalp has never yielded to treatment It has been under control but not cured

I mention these because they are idiosyncrasies of my own physical condition Otherwise I apparently was quite normal I had passed specialized medical tests in addition to the standard tests given by the Navy before going down

Another area of health I would like to discuss is the problem that came up when the common cold was introduced at the Pole Station There was a high degree of contagiousness suggesting an epidemic Although we eighteen men at the Pole Station living in isolation for 8 to 10 months had become immune to one another s cold germs we had apparently no resistance to freshly introduced exposure to colds The sequence of the sickness and the severity of the colds may prove of interest

Horvath This has been the observation made by all previous Arctic explorers

Siple I disagree that our experience was typical

Horvath Melville reported it at the North Pole It was reported by Stefansson * When the new visitor arrived and he had a cold a

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*Personal communication

similar phenomenon spread with tremendous rapidity to the rest of the group

Siple Yes Dr Horvath, your remarks apply to the sort of se quence that happened on three earlier wintering over expedition with which I was associated Some of the men on these trips ha mild colds when the ships came in but no one as I recall ha fevers severe coughs, and persistence as compared to the situatio at the pole when the first plane load of men arrived after the winte night

Blair Another difference in Stefansson and Melville is that the may have been interested primarily in the native aboriginal popu lation The Eskimos were hit hard These were people from Littl America who had had cold virus with self immunization to a larg degree The self immunization then broke down to a new reinfe tion by cold virus I think that is the point you are making

Siple There were at least two factors which differed from the ci cumstances involving the colds on the earlier expeditions At th pole I assume that the high altitude and low temperatures (−60 F at the time of the epidemic may have contributed to the severity c the illness With one partial exception everyone was affected and in some cases the effects lingered as long as 6 weeks until the me reached warmer climates on their return Again by contrast th colds on earlier expeditions were confined to a small minority an only lasted from 1 day to perhaps a week

This past year one man from Byrd Station was whisked out of th Antarctic and rushed right up to the Arctic He died a few days late He was the only Negro we had in the scientific group This ma had been living at an altitude of 4000 feet Possibly his case as wel as my own was caused by the virulence of the Asian influenza u combination with the colds I don t know

Fremont Smith Was it chest cold or head cold ?

Siple It was a combination Our doctors believed there were a least three types of colds that were involved Let me give you som of the pertinent excerpts direct from my diary notes Our first con tacts were with people who flew to the pole around 26 October

Burch Did you send nasal washings for bacteriological studies You could have stored the washings easily at the pole

Siple Not in my own case I think Dr Taylor may have take some samples of this type from some of his Navy personnel He mad some special scrapings and did some work of that nature

Burch Of course Asian influenza could be identified

Pace Capt Charles E Meyers of the Naval Biological Laborator

ollected nasal washings from Deepfreeze personnel last winter These washings have been brought back and are being run through for identification of organisms present

Siple To continue with notes from my diary On October 17 our first mail was dropped This was a big boost to our morale There was no indication of colds as a result of opening the letters et cetera

The mail was brought in very fast and was still warm from packing at McMurdo Some of the letters were as recent as a week from the United States So presumably we could have introduced germs but no one apparently had any cold symptoms as a result of the mail

On the twenty sixth we suddenly got 16 extra people in the camp because the plane they had come in could not take off again due to engine trouble Many of the newcomers were suffering with colds and other related symptoms Within a day or so the majority of them became worse and some were down in bed

Commander Witherell who was one of the leaders in the group has had a continuous cold Dr Hauck the Navy medical officer that came in with them had a temperature of 102°F Bierring who was one of the radio operators lost his voice and was sent to bed Almost all of the plane crew of the XV 6 were down —and I listed the names of the people here

On the thirtieth of October the majority of the wintering group (first 18 men to spend winter at South Pole) had their Asian flu shots This was four days after the arrival of the newcomers They brought the inoculations with them The new wintering group had their Asian flu shots before arrival About half of our winter party had an enervating and general sort of achy reaction from the shots Tuck and I worked two and one half hours in the snow mine that same afternoon we had received our shots I felt rather lousy the next day and took some time out Remington Osborne and Floyd Johnson especially felt worse than I did They had to go to bed from the reaction to their shots

All but one of our men had the shot At the end of one week nearly all of the newcomers had been sick in one way or another and were recovering Almost no one of the first year wintering group had any real symptoms of a cold Today and yesterday however the cold began to take effect This is the third of November my 338th day in residence at the Pole They came in on the twenty sixth of October

Several of our group are under the weather—MacPherson Dr

Taylor Remington, and myself I worked out of doors and in the snow mine about four hours today This evening, while I was leading a discussion, I nearly lost my voice It came on about 7 p m and steadily got worse It began yesterday and possibly even the day before, as a feeling of tightness and dry cough deep in my chest This morning the feeling was a little higher and little looser Tonight it feels like a full fledged cold Bronchial tract raw, and nose running Everyone in camp seems to have developed a cough'

I neglected to mention that after 26 October we had our first fresh vegetables (radishes onions, and lettuce) as well as milk and eggs

"There were several heavy jobs to be done It is obvious the new men are affected by the altitude far more than the old party'

We weren t actually conscious of this difference in ourselves until we began to do man hauling, and things of that nature

On 5 November I wrote 'My 'cold started in earnest yesterday and today I used practically a whole box of Kleenex It is rough to get a 'cold when one's nostril edges are raw from freezing

To begin with, Remington is down with his 'cold' Jack Tuck seems to be a day or so behind mine Arlo seems to be a bit later Dr Taylor is improving Ed Flowers didn't get a flu shot and doesn t have a cold yet"

Fremont Smith Ed Flowers is the one who didn't get the injection?

Siple Yes Flowers also was the man who was rarely ever sick He was I guess, busy or purposely avoided taking his influenza shot I think he didn't want to bother with it and, being a civilian was able to refuse it The rest of the civilian scientists all took the shots The military people had no alternative

On the eighth, I wrote, I am virtually all over my 'cold , Jack and Arlo at the height of theirs John Guerrero is feeling better Moose Remington is still down He had to go on antibiotics because the cold went into bronchitis coughed blood didn't have sore throat that characterizes about half the cases" In other words, half the cases had sore throats On 18 November, I wrote I have been feeling so miserable with my cold that I have not done well with my notes The course of my cold' has been quite peculiar Apparently I got more than one type of infection Back around the first or second I began to feel a tightening in my throat and chest and on the evening of the third I lost my voice, and on the fourth and fifth I had a heavy head cold I snapped out of it fairly well I took aspirin, and a rest but had a dry hacking cough My throat was not

sore I worked in the snow mine the next week and was outside some especially on the tenth and eleventh I did a lot of digging in the cold Being outside or down in the snow mine means exposure to temperatures of −60°F

The afternoon of the eleventh my throat began to get sore and my voice husky By Wednesday evening the thirteenth my throat got worse and after radio contact with home my voice nearly went out Since then for the past five days my voice has been very bad My throat is raw coughing moderate head cold returned discharge heavy whitish yellow from the nose yellow and greenish from the throat Nose bled three or four times I have used aspirin and antihistamine Several nights I have used some sleeping pills also tried a variety of things for my throat including salt gargles and things of that nature

The doctor has been giving a cough syrup to the men made up of alcohol codeine and belladonna I believe Also tried some odds and ends of liqueurs for throat relief Somebody had sent in some sample bottles of Southern Comfort and Irish Mist

Today my voice is slightly better With the exception of Ed Flowers everybody in camp has had varying degrees of cold flu or whatever it is

Siple I will continue to read from my diary in the order of severity of reactions Remington was down several days Bronchial bleeding very weak used antibiotics

Guerrero down several days heavy cold looked bad Retching cough

Remington stayed on and improved at the Pole Guerrero did not improve until he was actually out and in New Zealand for several days He went out considerably ahead of Remington

Dr Taylor fever down several days looked bad inner ear infected

Tuck exceptionally bad cough used sleeping pills and medicinal alcohol

I put myself as fifth Loss of voice long duration I won t bore you with the full details but it was almost 6 weeks before I recovered the use of my voice I was in New Zealand before I lost the last feeling of tightness in my throat I think part of my trouble was the fact I refused to take time off during the first symptoms I did rest but beyond this being in a leadership role I could hardly give up and take time off I think this may have aggravated my own case

Landolt had a long siege down some

Hansen was down several days

Waldron down several days but did not exhibit prolonged symptoms

Osborne our Navy carpenter was a hardy individual but had a severe siege but brief Down a couple of days but did a great deal of outside work

Havener our mechanic had a short siege which lasted only one day and he was outside a great deal

MacPherson radio operator had a long siege but not too severe

Segers the cook apparently a long period but didn t miss any work

Floyd Johnson meteorologist took it in his stride and kept working

Earl Johnson utility man always had a cold coming on but it didn t turn out to be very bad He lost one day of work

Dickey electronics technician got a cold later than the rest but not very severe

Benson scientist had a light cold sore throat just in the beginning

Hough scientist apparently no cold but admits to a few of the symptoms

Flowers meteorologist claimed no cold and no symptoms—the only man who didn t take the flu shot However later on in the notes all of these men had still more symptoms and Flowers lost his voice for one day and had at least a brief cold but his was not as severe compared to the others

Talbott How many of the group that came in and stayed for dinner were so miserable that they had to take to their beds?

Siple Nearly all of them Some had fever The plane crew after coming in did not have much sign of colds but they must have been catching them They had come from the United States quite recently They worked long hours outside trying very hard to get the plane going again and were breathing −60°F air

Horvath Was it not partly due to the altitude?

Siple Yes as I mentioned earlier I believe the high altitude may have had some effect

Horvath We are sort of losing this major point now I think you ought to be very clear Maybe these people were suffering primarily from altitude sickness

Siple No I don t believe that altitude alone caused the difference When our own party arrived the year before there were minor cases of altitude sickness shortness of breath headaches etc These were not the same kinds of ailments I refer to When the first visitors

arrived we had an epidemic of colds or something closely related o them

Horvath How many visitors came in besides this particular rty? You emphasized one visit Were there other visitors who ne in and were they followed by the same pattern of events? Is a rare episode?

ple This was on the occasion of the arrival of the first visitors y arrived 8 months after our last outside contact It was 2 weeks re before other visitors arrived That is the reason I bring up articular story As far as I know after the first siege of colds the men got well and there were no new colds afterward Apparently the men became immune to further colds Dr Pace you were there later on Did you get into the pole itself?

Pace Not to the pole

Siple At least you were in Antarctica after I left and may have heard what was happening I was not aware of further sieges

Pace It was this first burst that was so striking

Siple I felt it might be of significance to note this initial exposure introduction of diseases

Behnke For the most part the illness was afebrile?

Siple About a quarter or less had fevers

Behnke This is an old story in submarines where three fourths of the crew at the beginning have colds usually without fever which last about 2 weeks You made the point Dr Siple that the illness was about 6 weeks in duration

Siple After 6 weeks some of the men still had aftereffects of their colds

Behnke Were the quarters overheated was the air very dry?

Siple The air of course was dry

Behnke Was there smoking Was there cooking? Were there grease fumes?

Siple In the mess hall yes

Behnke Was there oil that was irritating?

Siple A moderate amount Half of the men in the camp were smokers

Behnke With poor ventilation there could have been substances in the air that might have been irritating

Siple In one building more than the rest

Behnke Were trace substances in the air collected perhaps on activated carbon for subsequent analysis?

Siple I don't know about that We had to isolate the mess hall by wall from the radio and weather station in the same building

because some of the smoke and grease from the cook stove was ca[u]sing malfunction of both the meteorological and radio equipmen[t]

Behnke Anyway the air was very dry inside and the humid[ity] perhaps was below 30 per cent?

Siple I am sure it was probably lower than that most of the winter Probably it was way below 10 per cent

Behnke I think this is an important consideration

Henschel Do you have any information from the experience this last winter at the South Pole? Has any new group been in there yet?

Siple At this date no It is just past the middle of the winter night for the second group at the pole

The health of the men during the winter was very good We had only a few ailments—one had hemorrhoids and another had a recurrence of amoebic dysentery acquired out in the Southwest Pacif[ic] in World War II Another had an undetermined chronic leg ailme[nt] which developed before he got to the pole These were about all [we] had Even tooth ailments and ailments that had plagued other ex[pe]ditions in the past claimed only one victim

Henschel It will be interesting to see what happens when the group flies to the polar station

Siple One of the reasons why I thought it might be significant t[o] draw attention to the nature of the Antarctic stations is that these annually isolated groups are continuing for at least another year or two and therefore they provide an opportunity for designed experimentation

Fremont Smith There are six etiologies possible and they have already been mentioned however we couldn't possibly determine which ones operate or how many re enforce one another To list them allergy dry cold air irritating substances in the air infection which could be virus or bacterial or both psychosomatic problems and response to vaccine Some would be inclined to suspect one an[d] some to suspect others some of us might think several interacted

I don't think we can possibly do more than list these and perha[ps] plan to work on them in the future A really careful virologic bacteriological study is necessary as well as studies of air contaminants I don't know how the allergy problem will be solved On[ce] an epidemic of this sort has started in a group a number of people will come down with the same symptoms on a psychosomatic basis It is a very complicated little problem and we cannot do more than list the potentialities at this time

Siple Of course I have not been attempting to give a scientific report I have simply been trying to give an account of what hap[pened]

pened to a group of men living under the most severe conditions

Fremont Smith These are raw data you are giving and this is very important to remember

Siple That is right I haven t attempted to analyze them nor do I feel myself capable of doing so

Travell Do I understand correctly that this upper respiratory infection was not unique to the wintering group but that the visiting group also had essentially the same thing? If so the long period of lack of exposure to these particular organisms probably couldn t be much of a factor

Siple I think this is probably correct I tried to keep track of the number of days and relative severity as the men became ill They had been living in isolation and in good health when suddenly a contagious disease was introduced at a known time

Travell You had a control group in the visitors who arrived

Fremont Smith These visitors did not have a clean bill of health nor had they been isolated but they had the same infection and symptoms as your group did?

Siple These visitors were themselves a heterogeneous group and many of them had colds when they arrived

Fremont Smith They behaved the same way your group behaved Therefore it wasn t the fact that your group had not been previously exposed because these people who had been previously exposed had the same symptoms

Siple I don t know whether the duration of their coughs and other symptoms lasted as long as ours or not or whether the results would have been equally severe for both groups at another location

Travell We saw a great many of these acute respiratory infections all through the United States during the same period

Behnke The group that came to Antarctica by boat 32 years ago did not have the same symptoms This earlier group was subjected to practically the same conditions that you have described but the members did not have the same symptoms Is that correct?

Siple Not at all the same symptoms In fact during the first 10 years of our work there we unwittingly and erroneously spread in the literature the belief that it is virtually impossible to catch a cold This became a popular concept We were at that time amazed at being able to go out and expose ourselves to unlimited environmental cold stress without resultant ill effects The first real colds that I ever saw in the Antarctic occurred during High Jump when we had many ships and literally thousands of men in the area We did not become immune to one another s cold germs during the time we were in Antarctica

The reason I bring up the subject of colds is that especially since World War II we have attempted to live under more normal conditions in the Polar regions and in doing so seem to be introducing new problems It is my own personal conviction that I aggravated my own acquired cold by breathing extremely cold air and perhaps the deeper breathing at high altitude may have had some additional effect

This as I say is a personal conviction rather than a scientific statement because I don t recall ever having had a cold like it in my life

Taylor I wanted to make a remark about our experience at Mc Murdo Sound which was at sea level The people who came in to break our isolation suffered equally as far as I could tell with us with regard to respiratory infections The situation is perhaps a new one in that an isolated group is visited by people who leave the States in late summer fly to New Zealand where it is early spring the season of respiratory infection there and then within 12 hours fly into the Antarctic These were summer human beings going in to the Antarctic who had been exposed to late winter and spring infections in New Zealand and then they went into an environment which obviously is favorable for the propagation and symptomatic intensification of respiratory infection

Montgomery What was the longest time you were without any visitors Dr Siple?

Siple Around 8 or 9 months—from about the middle of February to 26 October

Blair There is one very interesting thing you haven t brought out as yet When you first took your group of sixteen to the pole what was the pattern of the infectious cold upon arrival? In other words, how long did it take for all of the colds to die out? Did you have colds when you first arrived like these people when they first came in to the pole? Did the colds gradually die out with recurrences? Just what was the clinical course?

Siple During the first year the personnel came in three separate waves The first group a 22 man construction crew arrived in late November after spending a year at McMurdo An Air Force Sergeant and I joined them as recent arrivals to Antarctica We had had colds at McMurdo but no one as I recall acquired a cold at the pole that first summer The construction crew and the AF Sergeant left the pole and the first half of the winter party arrived about the beginning of the year The third group the remainder of the winter party arrived in February

Each group experienced shortness of breath due to the altitude especially during heavy exercise or when breathing was restricted by leaning over

One man of the construction party had an ailment that appeared to be pleurisy At most the men were finishing off colds they had brought with them from McMurdo and there was no indication of reinfection My health notes make no reference to colds If colds were acquired at the pole they certainly didn't impress me sufficiently to be worthy of note

Barquist Do you think that frostbite can result in the lungs or that pulmonary symptoms can result from heavy exertion and deep breathing in severe cold?

Siple We assume that the cold air damage is restricted to the bronchial tract and that the lungs were never actually frostbitten

At sea level with temperatures generally below −60°F there were a number of instances where men would start off fast to ski walk fast or work hard then suddenly they would begin to cough and spit some blood This was interpreted as being injury to some surface capillaries along the bronchial tract No frosting apparently ever got down into the lungs Pain associated with breathing deeply during heavy exercise on treadmills can occur with temperatures as warm as −30°F Under more normal working conditions it is more apt to be around −60°F before it is evident

Fremont Smith What pain was there?

Siple There was a tightness or a sharp feeling in the chest A champion Olympic runner in Dr Talbott's wartime group ran on the treadmill in a cold room at about an 8 mile an hour pace He was accustomed to running day after day for long periods After he ran about 5 minutes at −30°F on the treadmill he got off because of an unusual feeling in his chest He put on an experimental regenerative type face mask and went on running for half an hour without further difficulty The mask warmed the incoming air As I recall the air entering the mask at −30°F was raised to about +60 F before being inhaled

Barquist When doing this heavy labor at −60°F and lower were you conscious of having to restrict yourself because of breathing cold air?

Siple One of the notes I have in my diary describes an occasion at −85°F We went out to man haul some empty fuel drums There were two of us at a time hauling on the sled Because two men were working neither one of them wanted to be the one who stopped first We were only going 100 yards With the combination

of cold, altitude, and cumbersome clothing, we were as deeply winded at the end of the 100 yards as I ever recall observing We coughed then and for a few days afterward, but we did not cough up blood This is part of the evidence that we used to show that the thermal capacity of the air at that altitude was not as great at −85°F as it had been at −60° and − 70°F at sea level, when a number of the men had actually received sufficient damage to cause bleeding

Fremont Smith After people have run hard they may have a cough that lasts for several hours Therefore, it is possible that this cough was a combination of high altitude and a resulting condition of cyanosis perhaps it was not necessarily cold It would be very interesting to see if this could be separated out

Siple When the second half of our men were brought to the pole they were pressed into service at once We started building the barracks within a few days and the temperature dropped to around −60° to −65°F Most of the new men had never experienced such low temperatures We worked outdoors 2 and 3 hours at a stretch for a total of 10 to 12 hours per day The camp sounded like one for consumptives Each time we went to lower temperatures there was an outbreak of coughing, but we seemed to adjust ourselves at given levels

Down in the snow mine, where the temperature was a constant −60°F we would work very hard and get winded We apparently got accustomed to the level of cold so that we didn't have to cough On the other hand, if we went outside into −80°F air, we would start to cough again

Fremont Smith Cold evidently does add to the other factor

Siple Apparently it does

Horvath Do you have any real idea as to the magnitude of the work levels? What was the caloric expenditure?

Siple I did consistent work at −60°F, using an ice ax, chipping ice out of the wall, shoveling the dense chips into a bag, and lifting the 100 pound bag out of the way The first bag had to go up 10 or 15 steps or be raised at least 10 feet

Horvath How many 100 pound bags could you fill and lift in an hour?

Siple Six per man—chipping, filling, and lifting This is an optimum to a maximum There were only a few of us who could keep that pace and go 2 to 3 hours without a break We would often go down as a team of two or three, working together This gave a little rest period and permitted us to alternate in chipping Using a pick ax was the most exhausting part of the job Such a work rate

might not have seemed heavy at sea level but at 9000 feet it was quite strenuous

Bass When men were voluntarily inclined to stop working out in the cold and come in what were the limiting factors which made them stop? Also was this different if the men were doing hard work in the cold? Was it cold or exhaustion or what?

Siple It was usually a cold spot that gave them enough trouble and made them decide to stop I have that answer here in the case of two men who went out to see how long they could stay at −95°F This might be interesting because it shows what they were wearing and what actually happened

Today as the temperature nears −100°F two of the men wanted to see how long they could stay outside They asked me to check them out The following notes show the general results

Herb Hansen His nude weight was 158 pounds He showed actually no drop in weight We weighed him again nude after he came back in although the other man showed at least a pound drop I think this was a bad weighing We didn t have a good accurate scale just an ordinary bath scale The dressed weight was 18 pounds when he went out and 185 when he came in Rectal temperature 99 4 when he went out and it was 99 0 when he came back Clothing weight was 26 pounds

He wore Army type 50 50 wool-cotton underwear He had on commercial lightweight socks 50 50 wool and nylon He was wearing on top of those one pair of Army ski socks He wore felt boots or bunny boots as they are known with two pair of felt insoles He had on wool shirt Army coat type down vest wool Balaklava type helmet Army type parka coat—this was with the frieze liner and a wool pile lined and fur trimmed hood He wore Army style Arctic mitts with wool mitt liners and a headlight The batteries for the headlight were under his clothing

Hansen went out at 8 30 a m and came in at 11 30 He was out for 3 hours Temperature range −94° to −97°F The wind varied between 12 and 16 knots Wind chill was −3100 to −3200 by the normal computation methods

Here are some of the notes of what happened to him He went out at 8 30 At 9 20 he reported that his shoulders and arms had cooled in about 30 minutes The lower part of his arms felt very cold in 45 minutes The big toe on his right foot was cold after 55 minutes

Horvath Was he stationary?

Siple He was walking around It was dark outside except for the

light from his head lamp He was just ambling around doing nothing but just moving I would say that his level of work output was about 150 calories per square meter per hour

His torso started to get cold around 9.50, but seemed no colder At 10 20, however, the wrists and hands were cold The feet got cold whenever he was not walking At 11 15 he reported that he was partly warm and partly cold The bridge of his nose was freezing easily His wrists were cold most of the time At 11 30, when he came in, it was because his wrists and toes and the heel of his right foot were cold

Hansen is accustomed to going out frequently at these temperatures Both he and MacPherson had been working all the previous night, so they were not fresh It was extracurricular activity

MacPherson, a Navy radio operator, was probably one of our least well acclimated or cold accustomized men at the station He often complained about the cold, and hugged the stove a great deal He outlasted Hansen because I think he simply wanted to win His nude weight was 158 Both of these men, if you notice, were almost twins for weight However, MacPherson lost 2 pounds while he was out When he came back in, his nude weight was 156 pounds, if we can trust the scales Clothing weight was 27 pounds The dressed weight checked He weighed 185 pounds when he went out and 183 when he came back in Rectal temperature was 100 4, and it was 99 4 when he returned

The items worn Navy cotton waffle weave underwear These were long handles," as we call them Cushion sole socks, Army type with the white Army rubber insulated boots, cotton fatigue trousers and Navy cotton shell wool lining This fellow was really overstuffed He wasn't accustomed to going out, and he put on everything he could get He also wore an Army wool sweater, an Army wool shirt coat style a one piece flying suit, alpaca lined, a scarf of wool, Army type, gloves, shell leather, with wool liners, and Arctic mitts Parka hood detachable, with fur trim Army pile cap, and a headlight, and batteries As I say, he had been out on night duty the night before which was an indoor job, of course While he was out a total of about 4 hours, the temperature ranged between −94° and −98°F The wind was 11 to 16 knots

Some of the notes regarding MacPherson are as follows At 9 20 his white insulated boots froze extremely hard in the first 5 minutes Wrists were cold in 15 minutes His feet still felt warm At 10 20 wrists were cold He got rid of the leather gloves, but it was a mistake

to have worn them in the first place; however, he had his own choice. His feet got cold but warmed up again.

At 11:15 the hands stayed cold most of the time, also the wrists and feet got cold when standing, but warmed up when walking.

At 12:30 MacPherson gave up and came in because he was too tired. He had been up and working for 15 hours, as had Herb Hansen, when they started. He said, as long as he kept on the move his hands and feet both stayed warm.

At 10 p.m. I talked to both of them. They had gone to bed in the meantime. Herb complained that he slept poorly and had a head

Davis: You mentioned the fact that the boots froze.

Siple: This is the rubber insulated type. You know them perhaps as the Korean boot, but this is the Army Arctic version. They will freeze hard at from −65° to −70°F. I wore them at −100°F, and they were really just like iron.

Lyman: How do you walk in them if they are frozen?

Siple: I had to leave the tops loose. I had a red ringed mark around my ankles after my walk in them. Although this experiment yielded little that was new in respect to the protective value of clothing or relative to exercise, it did provide proof that two men were able to remain out of doors, on the move, for an average of 3½ hours in temperatures in the −90's with no great discomfort in 26½ pounds of clothing, and drop in body temperature of about 1°F and a water loss of 1 to 2 pounds.

Bass: Did they shiver much?

Siple: They did not complain of shivering. Because of the amount of insulation we wore, shivering was inhibited. Later, at New Zealand, several of us from the Pole Station went out in temperatures of around 55° to 60°F at a seaside resort close to Littleton. We had all just changed to our light civilian clothing for dinner, and suddenly stepping out from the hotel we all immediately started to shiver. We laughed because it was the first shivering we could remember experiencing for many months.

Fremont-Smith: The rate of change of temperature is very important for shivering.

Siple: Definitely. If you don't permit a temperature gradient in the skin because of the contact with thick clothing, you don't get the shivering impulse.

Keller Did you ever try to determine the amount of exercise such as a 100 yard dash, that would maintain heat balance?

Siple No, but working down in the snow mine, no matter what amount of clothing I tried to peel down to, I could not stop sweating the first hour or so I was there After this, I then got unmercifully cold and not because of wetness for I think I have as good clothing discipline as anyone I went through the sequence whereby for the first hour I could work with comfort, the second hour or third hour I became uncomfortable and began to get cold toes heels, ears, and an occasional frozen nose Of course, there was no wind down there

Burch Will you describe the mine? How deep was it?

Siple We dug forward 275 feet and downward 90 feet We went down on an 18 to 20 degree slope

Burch How large was it?

Siple It averaged 8 or 10 feet in cross section The narrowest cross section was 7 square feet and the biggest was probably 20 feet high at certain places We had great chambers along the mine We brought the snow up on little sleds which we were able to drag up with a winch

Burch Were you producing the mine for specific purposes?

Siple Mainly for glaciological study and for camp water supply We combined the two in order to get the depth One man had been given the job of digging this pit Obviously he couldn t do it so we had to assign it to everyone

Burton I think I can settle the question about "freezing the lungs" The following is from experiments I did with Harry Armstrong (General Armstrong of the Air Force) (1) and with Dr C E Hall (now president of the University of Western Ontario) in 1940 We studied the effect of breathing air which we supplied from −10° to +40°F We measured the tissue temperatures all the way down the respiratory tract of dogs, the temperature of the lung tissue, and the difference between blood entering and leaving the lungs

We found, perhaps to our astonishment at first, before I got down to calculation, that it made no detectable difference to the temperatures below the cricoid cartilage, whether the animal was breathing air of −40°, or +40°F

Then, when we had completed the research and had the measurements on these temperatures, I worked out the physics Taking the minute volume that could be breathed by a man, even in heavy exercise, and assuming that there was no air conditioning above so

that the lung blood volume had to supply all of that heat I found that the lung blood would drop by less than 0 1°C So I think there is no doubt at all The fact is that air has such a low specific heat particularly high altitude air that there is no possibility that one could suffer a serious change in the lung tissue temperature which if it happened of course would produce very profound physiological effects One does not therefore have any possibility of physiological changes due to temperature changes of the lung tissues or the exchange of oxygen etc

We have heard some clinical evidence about the effects of breathing very cold air particularly in hay fever victims I think that these effects must be ascribed to reflexes from the upper respiratory tract which is seriously cooled There is no possibility that the lungs are cooled appreciably or frozen

Barquist I raised the question because the Army Information program which publishes pamphlets to be given to troops within the last month has published an orientation pamphlet about the Arctic It still contains despite this work you mentioned a paragraph about the danger of frostbite to lungs

Horvath In 1943 the Army supported a research program by Moritz (2) who in 1945 reported on the same work Dr Burton is talking about indicating that while breathing air at −60°F or +40°F adequate adjustment could be made

Marks Is there any difference in effect between mouth breathing and nose breathing?

Burton Not in this respect I am just guessing This was on dogs The fact is that the air has all the heat it requires to rise in temperature to 37°F before it gets down to the cricoid cartilage

Before we started this work everybody told us that it was well known that horses lungs freeze in the Arctic So we wrote to our Royal Canadian Mounted Police and to the Ontario Veterinary College for information on this I got a very definite reply that the RCMP had been working horses for many many years in the Yukon at −40°F and had never had any indication of lung freezing The only thing that happened was occasional freezing of the nostrils This is another example of one of the things that were well known

Burch Was there desiccation of the upper respiratory tract?

Burton There certainly was when very dry air was inspired

Burch The cold air is dry Expired air is 88 to 90 per cent saturated with water

Fremont Smith Your question is whether there is enough mois

ture in the air by the time it gets down to the lungs What about drying in the lungs themselves?

Burton There, again, the water exchange takes place earlier In fact, I am a rebel I don't believe there is loss of vapor from the lung itself, but rather from the respiratory tract above the lungs

Siple I don't know whether there is any state of drying in the lungs, however, there is certainly a lot of visible water vapor expelled at every breath

Henschel Recent work (3) indicates that the moisture loss in the Arctic is actually not very great Paul Webb of the Air Force and our laboratory has measured this loss in experiments at Churchill by freezing out the water in expired air The total water loss is not tremendous

Burton Let's get the physics correct Even if saturated air at this room temperature were inhaled, it would still need to be raised to a temperature of 37°C and require further addition of water to become fully saturated

So, even if you are breathing room temperature air which is saturated you still have to supply the major part of the moisture in expired air If, however, you are breathing very dry air, you have to supply a little more moisture It is not a major addition, perhaps 20 per cent more, if the air is bone dry We must remember we must saturate the air at 37°C, not merely saturate it at room temperature

Fremont Smith Nose and mouth breathing might make a difference in this saturation Nose breathing presumably would be a better saturated mechanism than mouth breathing

Horvath Actually, that is not the case It makes no difference Proetz (4) wrote a book on the physiology of the nose which I think everyone should read He gives a tremendous number of measurements of temperature and approximate measurements of relative humidity in the nose and other parts of the upper respiratory tract This information has been known for at least 40 or 50 years It is concerned with the effectiveness of adding water vapor in the upper part of the respiratory tract, it is also concerned with the ability of that part of the mucous membrane to warm the air from very low temperatures to approximately the middle 30's and also to cool air from the upper 40's and upper 50's down to the middle 30's All of this occurs, as far as anyone can tell by the measurements made, in the upper part of the respiratory tract In the trachea and bronchioles nothing really happens except the beginnings of gaseous exchange

Fremont Smith It is hard to see how deep breathing through the

mouth can result in as effective a saturation with moisture as similar breathing through the nose It seems to me this is a big column of air that has no chance to be saturated by water at all before it reaches the uvula

Horvath It is a tremendously effective surface I think we ignore the capacity of this system

Fremont Smith Yes but this is the surface of a wide open mouth and throat as compared to narrow passageways in the nose and the turbinates This is very difficult to accept

Siple In fact the nose gets full of condensate it drips and one has to breathe through the mouth

Behnke Symptoms which you described could be reproduced in this room during the course of oxygen inhalation in which the equipment was surrounded by dry ice Under these conditions one experiences nasal congestion and rhinorrhea

With reference to hyperventilation in submarine experiments in which Dr Pace participated carbon dioxide accumulated in the ambient air to values as high as 5 per cent and ventilation rate increased to 35 liters per minute We certainly became cold didn t we?

Pace Yes I have noticed this with carbon dioxide breathing and others have too There is a definite feeling of cooling apparently as a result of the hyperventilation

Fremont Smith A feeling of cooling where?

Pace Just a sensation of being cold This was a common complaint

Fremont Smith This could be drop in skin temperature Vasoconstriction of the skin could make you feel cold

Travell Does the core temperature fall?

Pace This is something I think ought to be investigated some time In the course of running the CO response test which takes a period of an h[illegible] CO_2 subjects characteristically

Fremont S[illegible] f skin temperature change and not body temperature

Siple I have only scratched the surface of these problems Some of you may be interested in the excessive loss of weight noted when we first began to live at the pole and the occurrences of aches and pains possibly caused by some slight anoxia created by reduced circulation in the low temperatures as well as reduced availability of oxygen

The comparative losses of weight between the construction crew

and the wintering party were rather interesting Dr Taylor may want to mention something about the weight loss of the construction crew that had had a year's acclimation at low altitude

Taylor The average weight loss was 10 pounds in 6 weeks

Siple The wintering party undergoing acclimation right at the pole lost more In my own case, I could stand the loss of 39 pounds—dropping from 250 to 211 pounds without dieting But, more drastic was the case of another man who weighed 162 pounds on arrival and lost 30 pounds He was one of the individuals who was outdoors and consequently exposed a great deal during this period He was a small man and he lost that much There were a number of others that lost close to that amount of weight This, I think might be caused in part, at least by the altitude This is not a typical occurrence at sea level, apparently

Also, Dr Adams, who was with the Fuchs Expedition, indicated that his people experienced some of these joint aches at sea level and he attributed this to sleeping cold, which restricted their circulation during sleep We got the same symptoms, however, at altitude while working, although we slept warm at night and were comfortable for the rest of the day

Travell Was the weight regained?

Siple Some people regained very little. I gained back about 15 pounds of weight lost Most of the men gained during the winter time, when they were indoors and warm most of the time

As a postscript, we noticed, in quite a number of men, that the skin of their fingers split when they were working Part of this was probably caused by heavy work but also, again, I think it may have been caused by the drying of the skin Fissures opened up and got very sore they stayed sore for quite a while, too

Burch Did you try ointments?

Siple We used lanolin and this helped The splitting was symptomatic of about four or five people in the camp They were individuals who were exposed quite a bit, or had excessive contact exposure to their hands

Behnke What was food consumption in terms of calories?

Siple I indicate in my notes at one point that I found the food appeal wasn t too high during the heavy cold labor period, which would imply that maybe I wasn't eating a great deal

Behnke About 5000 calories?

Siple I doubt it was that much

Behnke Did you measure it?

Taylor No we didn't measure it but our supplies were for 6000

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MEDICAL EXPERIENCES AT McMURDO SOUND

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PERMIT ME to describe some of the medical experiences at McMurdo Sound while I was there By way of introduction I might go through the organization of the operation of which I was a part and explain my role This of course was Operation Deep Freeze I the initial phase of the Navy program in support of the International Geophysical Year

In the summer of 1955 my assignment was to spend the winter as the medical officer at a base to be built at McMurdo Sound This was the year before the Antarctic year which Dr Siple has just described

We had the problem in the planning phase of the operation of assembling all the materials and the supplies which would be necessary for the construction of two bases in the Antarctic one at McMurdo Sound and another at or near the sites of the former Little America Stations These were to be the advance bases of Operation Deep Freeze

We were to go into the Antarctic in December of 1955 establish a camp for unloading the supplies from the ship and then go ahead and build the quarters and the facilities for our winter camp

Ours was not a scientific expedition It was purely preparatory to the arrival of the scientists the year following for the International Geophysical Year Our personnel were mainly naval officers and men with a few Air Force enlisted men and at each camp there were one weather bureau observer and one civilian photographer

At McMurdo Sound we were to build a camp to house approximately ninety men over the course of the ensuing Antarctic winter the winter of 1956 In addition during that winter we were to build a runway suitable for wheeled Air Force Globemasters (C 124 s) so

that they could fly from McMurdo to the Pole in the following summer to drop by parachute the supplies for the base which Dr Siple has described earlier

Moreover during the winter we had to prepare in the camp all of the material and supplies for the construction of the South Polar Base These had to be assembled and packed and parachutes made ready for the drops from the Globemasters

We had also the problem of carrying out a training program or whatever was necessary for the twenty four men of the construction party members of our wintering over group who were to go in to the South Pole and actually build the base This was our mission at McMurdo Sound

The Little America Station was to prepare similarly to build the base planned for Marie Byrd Land at 80 South Latitude and 120° West Longitude The construction of that Marie Byrd Land base is another story a really masterful achievement of trans Antarctic operation But my experience was strictly in the McMurdo Sound area and that is what I shall talk about Planning for both the Little America and McMurdo Sound bases was carried out at the USN Construction Battalion Center at Davisville R I There Dr E N Ehrlich of Detroit who was to be the medical officer at Little America and I with the advice of the Task Force Medical Officer and the assistance of supply personnel at Davisville proceeded to assemble material which we anticipated we would need for the winter we were to be alone Dr Ehrlich was going to have about sixty people at the Base and I was to have about ninety at mine Since no guide for outfitting was available we based our selection on anticipated needs We used a modification of a mobile construction battalion medical outfitting list for the conditions which we thought we would face

The most important unique aspects of these two bases were first that we had by antarctic standards a large number of men and second that it would be impossible for us to evacuate sick or injured personnel between March and November In general we took a small quantity of everything we thought we might need but we did not take medications to treat amebiasis and tropical diseases which was unfortunate as Dr Siple s remarks showed Our guesses were good and we had no really serious deficiencies

In addition to our supplies we were provided with a field x ray unit for use in the sick bay This is a 15 ma unit a standard Armed Forces field item Actually it turned out to be quite satisfactory for

he uses to which we put it We were also provided with a field autoclave fired by gasoline and this was very useful We had instruments anesthesia machines and other equipment for small operating rooms

There were to be two hospital corpsmen at each of the stations that is to say two at Little America and two at McMurdo Sound We had in addition a Navy dental officer who was assigned to spend the winter at McMurdo Sound Dr Ehrlich did not have a dentist at Little America

So during the summer of 1955 we assembled the equipment and supplies These were packed in Davisville and put aboard the ships as cargo In addition for Little America and for the base at McMurdo Sound we packed a field kit which we expected to use during the initial phases when we went ashore from the ships in December of 1955

The plan was that the advance party consisting of about forty Seabees would proceed in mid December from New Zealand south to McMurdo and there go ashore establish a tent camp somewhere in the neighborhood of Capt Robert Scott s old bases on Ross Island and get established so as to receive the supplies and materials for the main camp We were all quite inexperienced not only about Antarctica but about any kind of cold weather operation We hardly knew what we were getting into As far as I know no one had wintered in the McMurdo Sound area since 1915 when Shackleton s western party spent the winter there It was with a sense of excitement and anticipation that we moved south from New Zealand

The McMurdo Sound base is located on Ross Island which is directly south of New Zealand about 700 miles north of the pole The island is of volcanic origin and rises steeply from the sea to the summit of Mount Erebus an active volcano over 10 000 feet high The island lies at the southwestern corner of the Ross Sea and is separated from the mainland by McMurdo Sound an arm of the sea about 45 miles wide The Ross Sea and the Sound are usually open during the Antarctic summer and ships may penetrate during the season as far south as Ross Island To the south and southeast extends the Ross Ice Shelf on the northern edge of which 400 miles east of McMurdo is situated the Little America Station New Zealand the nearest inhabited land is over 2 000 miles north

The first actual operational phase in the Antarctic consisted of our going ashore at McMurdo establishing a base camp and prepar

ing ourselves to work We didn t know what the temperatures w(be like nor did we know how much ice there would be

When we arrived in an ice breaker at the ice edge in McMu Sound, we found that the Sound was still completely frozen] was 20 December The ice breaker could not come closer than ab 45 air miles, about 60 miles as it turned out, across the surface the ice from the site which was eventually chosen for the camp

Our camp the McMurdo Sound base, was located on Cape Ar tage, which is the southernmost tip of Ross Island, and it is witl sight of and virtually the same location as the wintering quarters Captain Scott s expedition in 1901 But this was 45 miles from edge of the ice, which posed a tremendous problem in transport material to the base site, and a tremendous health hazard or haz of accident and injury to the personnel engaged in the operation

We went ashore with our tents and the ships left us, sailing c to go on station for aircraft flying in from New Zealand We th undertook to move some supplies in from the edge of the ice

The first event of medical significance was a plane crash whi occurred at the ice edge about 60 miles from the camp At that tin we had the first of many injuries which the men suffered during ou year there One man had a broken ankle and a broken knee Ther were no ships in at that time, so we had to bring him back to th base at McMurdo Sound, and try to care for him in a tent, fortu nately under summer conditions

Blair Did you notice any difference in the way these injurie healed, the rapidity of healing as compared to their reparativ qualities in the States? The suggestion in Greenland was that ther was some slower healing or a slower reparative process

Taylor I had heard this and I observed the rate of healing o lacerations and incisions etc , and also the rate of healing of broker bones I couldn t see that there was any delay Although cracks of the skin due to drying and thickening and chapping tended to stay open in men who had to work outdoors a lot

Burch What was the weather at that time?

Taylor In the summer the temperatures at McMurdo ranged be- tween 25° and 35°F On clear still days the air temperature would get up to about 35°F, once even to 40°F The lowest temperatures during the summer months were about 25°F I think the lowest temperature I had in the hospital tent, before we moved indoors into the permanent winter quarters, was 19°F in February, which was getting on toward the fall So the temperature conditions were not severe However we did not have floors for the tents and it was

very difficult to keep the temperature in the tent much above 40 F, which made the care of the injured men quite difficult

We had this casualty with us for about 4 days when the ships returned and we were able to put the men in the sick bay on board

After that until the departure of the ships all of the medical activities had the support of the medical facilities aboard the ships of the Task Force which were in McMurdo Sound So it was only during a few critical days that we were without proper medical support

We found at Cape Armitage a small shelf like plateau about 120 feet above the level of the sea which was clear of snow Hills to the east and north some 700 to 1200 feet in height afforded some protection to the camp site from the expected winter winds The ground consisted of volcanic debris the first 12 or 18 inches of which was loose rubble below this was permafrost The permafrost made grading difficult and prevented ditching or the construction of pit latrines The loose unfrozen foot or so at the surface however provided good natural drainage from the site with relatively little water actually running on the surface during the summer thaw

By the first of March we had fairly well completed our winter quarters Our sick bay consisted of half of one of our standard buildings The building was 20 feet in width and 48 feet long and was shared with the administrative officers of the camp It soon became obvious we did not have adequate room in the sick bay and from extra panels an extension was improvised which did give us adequate room for our wintertime operations

In spite of the long distances across the ice that the supplies had to be moved out of all the materials which had been packed in Davisville we lost only one small item which we really didn t miss The delivery of supplies was very satisfactory It was warm enough so that the freezing of perishable supplies was not a problem though toward the end of the summer and just before we moved into our winter quarters the temperatures did begin to go low enough so that things might freeze We kept these items in a tent heated by a Yukon stove and were able to protect the medical supplies in that fashion

As I said the medical department consisted of a dental officer Dr D J Knoedler two hospital corpsmen and me We had the sick bay set up and operating by the time the ships left on 9 March After that we were completely isolated except for radio contacts until October when the planes returned

Fremont Smith What was your total personnel?

Taylor We had a total of ninety three in the camp We had a higher proportion of medical department personnel than a camp needs as long as everything was going all right but when people began to get hurt or sick then we needed two corpsmen It seemed to me that in any camp with more than fifty men in which evacuation is not going to be possible for several months at a time it is almost a requisite to have in addition to the medical officer two hospital corpsmen available to spell each other These men should understand that they aren t going to be busy throughout the time and they should expect to be assigned to other duties as our men were In my experience when medical department personnel are needed there is no substitute

Blair Was there any special type of packing conditioning or winterizing of the supplies before shipping them out?

Taylor No we did not winterize Biologicals such as vaccines and other supplies that we were particularly concerned about getting either too hot or too cold we carried in our hands It is possible to do that with a small package and put it in a refrigerator if necessary on board ship

Burch You are an internist Would it have been better to have a surgeon than an internist for that type of work?

Taylor This was a volunteer operation I will say something about personnel later I am told that no surgeon volunteered ! wanted to go and yet it was with some misgiving on account of the fact that I was not a surgeon Looking over the situation as a whole I don t think that it was too great a misjudgment on the part of the Navy to send an internist there When we needed a surgeon though we really needed one I can say that

Burch Were most of the problems in the field of internal medicine?

Taylor Most of the problems were in the field of internal medicine and office practice psychiatry Both Dr Ehrlich and I were internists But as part of our preparation I was sent to the Naval Hospital at Bethesda to work on the orthopedic and surgical services for 3 months and Dr Ehrlich to the Newport Naval Hospital to do the same thing this gave us quite a good preview

Talbott Have you estimated what percentage you used of the total drug supplies you took?

Taylor We have usage rates on all the things we took But I can t cite them for you now

Talbott Was it relatively high or relatively low?

Taylor It was much lower during the winter than we had antici

pated but during the summer that is to say the second summer we were there it was much higher because we had in the camp many more people than we had anticipated In addition the problem of respiratory infection was more marked than I had thought it was going to be

Burton What supplies of blood did you take?

Taylor We took only that which the members of the party carried with them and we took crossmatching sera and recipient or donor sets to draw blood We also had a plasma expander dextran along with us I will speak about that in a moment

Behnke What about the probability of appendicitis developing?

Taylor That was of course a possibility I think that we were prepared to deal with that Dr Ehrlich had a case of appendicitis at his base and successfully removed the offending organ I had a case and successfully treated it with antibiotics

Burch Was one of the corpsman trained as an anesthetist?

Taylor Neither of my corpsmen had any kind of surgical training At Little America neither of Dr Ehrlich s corpsmen was an anesthetist or operating room technician I had a neuropsychiatric technician who was helpful My other man had no specialty but had experience with x ray and that also was very helpful

Burch Did he use a spinal anesthetic in the patient?

Taylor He started off with a spinal but it doesn t seem to last as long in cold climates or isolated bases as it does in the States so he then changed to ether

Behnke Were there dental emergencies?

Taylor If there were they were so efficiently taken care of by the dental officer that they never came to my attention Dr Ehrlich had two men at Little America whose dental problems he didn t feel he could handle He pulled several teeth and made some temporary fillings in consultation over the radio with the dental officer at McMurdo

After the ships departed the work of the camp continued and the buildings were completed We had five barracks buildings 20 by 48 feet in size The mess hall was 20 feet wide and 96 feet long There were two latrines each of which had hot and cold running water About 75 yards from the barracks buildings there was a large powerhouse in which the generators were located and here there were showers for bathing as well as washing machines and clothes driers

Due to the high wind velocities during the winter we had relatively little snow accumulation in our camp area Only where a

building or a pile of supplies rose above the ground was there drift ing Unlike Little America, our camp was not buried in snow

We stored most of our supplies outdoors All of the food was out side Though snow drifted around the food piles it was possible to bring food in at any time during the winter The medical supplies we stored outside, except for the freezable items Snow would sift into the boxes but, of course, there was no thawing, and if one took care to shake the snow out of the medical supplies, before bringing them into the sick bay area the open storage was perfectly satisfactory

The messing facilities were perfectly adequate for our use in the wintertime, when there were but ninety three of us, but in the summer, with 300 people in the camp they were quite inadequate We had to improvise quite a bit and did not come up to proper standards of sanitation in our messing facilities We didn't have dish washers and we didn't have water which was hot enough really to sterilize the eating utensils This did not concern me particularly in the winter but in the summer months when we had such overcrowd ing and many transients coming and going, I think the situation was hazardous It was really just luck that we didn t have a serious out break of some sort of gastrointestinal disturbance

The personnel of the camp, in the winter period, consisted of twelve officers of the Navy, two civilians, seventy six Naval enlisted men and three Air Force enlisted men who were there in the inter est of preparing the material for the drop at the Pole The ages of the wintering over people ranged from 18 to 52 The oldest man was Sergeant Dolleman of the Air Force who had been with Admiral Byrd He accompanied the U S Antarctic Service Expedition to the Antarctic in 1940 and had much experience in Greenland He was the only one in the camp who had had any experience with isolated duty in cold climates The average age of this group was about 28 years

Our water, of course, came from melted snow When we first went in in the summertime, we found a melt pool up behind our tempo rary camp which furnished palatable water and which could not be said to have had much human contamination We used this without any ill effect during the first summer In the winter, of course this froze and we had to melt snow for all of our water

Irving Did you sterilize the water?

Taylor No we didn t have any facilities for doing so Snow for melting was collected in an area several hundred yards to windward of the camp by a large hydraulically operated scoop mounted on the

front of a self propelled vehicle The snow was then dumped into melters located at the powerhouse and at the mess hall

In the wintertime we used about 15 gallons per day per man This technique of water gathering was quite satisfactory during the winter We had showers and washing machines with hot and cold water in both of those Then we had running water for washing hands in the latrines and of course running water in the galley In a couple of hours in the wintertime a man operating this machine could get enough snow to provide water for the camp for a day

In the summertime with more people to keep the camp supplied with water we had to have one man per day shift and one man per night shift operating the machine

Burch How did you dispose of the waste?

Taylor I shall tell you later The water was palatable there was a little volcanic ash in it when we melted down the snow and a little soot from the diesel fuel that would blow into the top of the tank but it was quite satisfactory We were few enough in number so there was very little possibility of contamination of the snow gathering area in the wintertime But in the summer the situation was considerably more hazardous because we were overcrowded there was always the possibility that someone would walk in the snow-collection areas Finally we exhausted the local supplies of snow and had to move back in the hills which made a longer trip for the snow gathering machine I spent a good deal of time worrying about the water as we had no facilities at all for purification Again I think we were lucky that we didn t have some water borne infection

Barquist What was the water temperature in your snow melter?

Taylor We heated the water again after it was melted I don t know what the temperature in the melter was

Barquist In the melters used in Greenland during 1957 the temperature would probably get high enough several times a week to kill vegetative forms of bacteria if any were present

Pace I believe a chlorinator has been added now

Taylor That was one of our recommendations when we came out and it certainly is worth while

Siple It probably very seldom got very warm because as soon as the snow was melted it was used and the melter was filled right up again with more snow

Taylor Certainly in the summer snow was going in all the time Our sanitation facilities consisted of latrine buildings with running water The latrine refuse was collected in 55 gallon fuel drums

which had been sawed in half and placed in position under the latrines As soon as these were filled, they were hauled out on a sled to an area about a quarter of a mile from the camp, down a hill, and onto the surface of the bay ice, where we anticipated that presently the ice would go out and these would go with them The refuse froze very quickly in the winter and was handled quite satisfactorily In the summertime, however, freezing did not occur, and this contributed a considerable hazard to the problem of refuse disposal Also, the latrine refuse dump had a snow cover in the wintertime which I thought was fairly satisfactory, in the summertime, though there was no snow cover

There are no insects in the Antarctic of known epidemiological significance So this was tremendously in our favor. There are, however, skua gulls in large numbers around McMurdo Sound These visit the refuse areas but then we have no way of knowing where they go This did constitute a hazard which we weren't able to meet at the time I was there

The drainage from showers and from the washing machines went out to the slopes that led down to the bay and, though it froze in the winter, it soon ran off with thawing in the summer and seemed satisfactory enough at the time During the winter, the men were very healthy, with the exception of injuries, some psychiatric disorders, and insomnia

Blair What type of preselection, if any, was there on the men coming in, from the standpoint of physical examination?

Taylor The announcement that was sent out in 1955, requesting volunteers, stated that a volunteer had to have the physical qualifications for submarine duty This was the basic physical requirement Those who were selected received their orders to Davisville and they worked there for several months during the summer of 1955 with the officers who would have charge of them in the Antarctic In addition to eliminate men who might not be qualified, Dr Ehrlich and I personally examined all of these men, giving them a complete physical check up and taking a history We also had psychiatric interviews of a screening type with all of the personnel who were to winter over In three or four instances, the psychiatrist recommended that we not put a man on the wintering over list

We departed from Davisville with 150 men who had volunteered to spend the winter at McMurdo Sound We all went ashore and worked there during the summer months, from December until March, so that we had an opportunity to see these men at work right on the scene Then, since we were going to keep only about

ninety in the camp we had the opportunity to send back to the States about sixty men Actually except in a couple of special cases after a man had been down there for the summer we gave him the opportunity to go home if he wanted to So we really had a double selection of volunteers There were about thirty who said they wanted to go home So we had about thirty more optional places *from which we could select men on the basis of performance at the* actual scene of operation This sort of double selection gave us a very good group of men with really only one man not fit for duty

During the winter my sick call would run from five to ten patients a day and those were patients whose names I entered in the sick bay log A lot of men would come by just to sit down and talk for a while I think you could consider this a sort of psychiatric sick call but we didn t often record these visits

The most serious problems were fractures We were able to deal fairly satisfactorily with the fractures we had with the exception of one man who fell from the roof of a building and severely fractured both elbows at the same time This occurred in April and we couldn t get him out until November This was one of the times when I thought the Bureau of Medicine and Surgery should have sent an orthopedic specialist however the rest of the fractures were easy enough to handle

Another serious life endangering situation was carbon monoxide *poisoning One man became unconscious while riding in a Weasel* which is actually an open vehicle but he was sitting so that the stream of the exhaust came right to him

Insomnia was epidemic I thought that it was due to a combination of homesickness with slight attendant depression and the disturbance of the sleep cycle which accompanied the disappearance of the sun We were able to deal with many cases of insomnia by giving barbiturates for say 3 nights which would allow the man to re establish the normal sleep cycle He would do all right for several weeks and then sometimes get out of phase again

The most chronic problem I had was a psychiatric patient a man about whom there was some question prior to leaving the States but who wanted to stay and whose services we needed We thought he could last the winter but we were wrong At the end of March he developed a frank psychosis and was unable to work for the rest of the year The patient s condition rapidly improved with the re*turn of light after the winter night and after only a short period of* hospitalization in the United States he was able to return to full Naval duty

Our experiences with this man were interesting. He was deluded and, occasionally, hallucinatory, but oriented with respect to time and place, and able to look out for himself quite well. He was very unpleasant to associates, and yet our facilities in the sick bay really didn't provide the opportunity to care for him as he could have been cared for in the usual psychiatric environment. In addition, if he had been confined, it would have been necessary to have a watch on him 24 hours a day, which would have been extremely consuming of the total man hours of the camp. So, we talked the situation over with the personnel of the camp at a meeting, and explained what the situation was as best we could. We told them we wanted to try letting the man go about the camp, he was to take his meals in the mess hall, though sleep in the sick bay. We asked the rest of the men to be forbearing, if not understanding, in their dealings with the patient. This worked very well. We had actually to confine the man to the sick bay only for about 4 weeks during his most excited period.

Fremont-Smith: This is the modern treatment of psychoses. This is the open door method which has been developed in Great Britain and we are learning from Great Britain fairly rapidly. At the last meeting of the American Psychiatric Association, the President said the most important thing we can do in psychiatry in this country is to liquidate our mental hospitals. So, you were forced to do something which is quite modern—quite proper, and toward which we will gradually move. I think we can learn quite a lesson from the fact this worked as well as it did. The confinement of mental patients, the way we are doing it with our so-called modern methods is really outdated.

Davis: The primitive races did exactly the same.

Montgomery: Did drugs help?

Taylor: I had some chlorpromazine but did not use it, as I was really unfamiliar with it. However, I think it was good for the morale of the camp in general to have this man. He served as an object lesson for many of us and was, of course, the butt of many remarks such as, "Are you saving that other rack in the sick bay for me?" My estimation was that this served as a rather useful outlet for the tensions of the rest of the camp.

Fremont-Smith: He pulled his weight, in other words?

Taylor: Yes, by not pulling his weight he did.

Hock: Did the men have enough to do to keep busy?

Taylor: Boredom was not a problem for us. We were extremely

busy all winter long First we had to finish building the camp then we had to pack the supplies for the South Pole Station and finally we had to build an ice runway clear the snow off the runway out on the surface of the camp

We were in the Antarctic for 14 months and there were 6 weeks during the darkest part of the winter when we were on an 8 hour day and 5½-day week But for the rest of the 14 months our men worked 12 hours a day 6 or 7 days a week It was a grueling experience for them Fatigue rather than boredom was our problem

Talbott Did you have another major assignment?

Taylor No I did not I served simply as the medical officer

Marks When they were on an 8 hour day how did the men occupy their spare time? Did they read? Did you have films?

Taylor When we were on an 8 hour day we had movies every night The rest of the time there wasn t much time for recreation We stopped movies for example when we really went to work on the runway except for one movie on Saturday night simply because we couldn t provide the men with 8 hours of sleeping time and have movies also We had an excellent library and the men read quite a bit We had music on records and tapes and they enjoyed this Every Saturday night we had an entertainment a happy hour in the mess hall at which time alcoholic beverages were served This seemed to release a good bit of tension About once a month the chaplain organized a little program and the men in various huts in succession put on a little show We enjoyed this a lot

Burton You didn t have any troubles associated with lack of female company?

Taylor This is the sort of thing you forget about I can t remember the emotions associated with that Things went pretty smoothly in general

Behnke Did you have any kind of weekly inspection or dress period?

Taylor We had no dress period and we maintained a rather loose kind of discipline I have had very little naval experience but my interpretation is that we relaxed distinctions between officers and men We ate in the same mess and sat at the same table The officers visited in the men s barracks however the men did not come into the officers barracks except on business They weren t forbidden to do so but they just didn t We were on quite a friendly basis Once a week there was an inspection of the camp by the camp commander

and once a week we had a sanitary inspection of the camp But the clothing, the uniforms, were not regulation at all We wore what ever we needed to keep warm

Talbott Did you relax to a first name basis between officers and enlisted men?

Taylor No We called some of the enlisted men by first names but they addressed the officers as "Mister" or 'Doctor' The two civilians were on a first name basis with everybody in the camp

As I contemplate being in an isolated station with groups of other sizes ninety three is a nice size to have It is a large enough group so that you don't have to see precisely the same person at every meal Thus, there was some variety in our conversation

Our living quarters were quite crowded In each of the 20 by 48 foot barracks, twenty men slept in double-deck bunks In each barracks there was a space reserved for a lounge with very pleasant furniture in it The sleeping cubicles had four men in each They were divided by semi partitions We were crowded in those barracks not because of the number of men so much, but because the clothing was so bulky that storage space was at a minimum I think this crowding probably had something to do with the insomnia It made it more difficult for the men to relax There were very few places actually to which a man could withdraw and be alone The library was quiet and a man could sit down there and be by himself We had a little chapel which also offered a retreat

Lyman How cold was it outside?

Taylor In the winter the standard temperature was between −30° and −40°F The average wind velocity was 15 knots but we had frequent storms in which the wind velocity reached 60 knots With the higher wind velocities, the temperature generally rose to say −20°F In the coldest period, the lowest temperature we recorded was −68°F on the runway 2 miles down the hill from the camp This was for only a brief time

At the runway, where the men were engaged in outdoor work all winter, the temperature ran 10 to 20 degrees lower than it was in the camp They were working outside at −50° to −60°F all winter long but the men were not doing heavy physical labor most of the time They were driving tractors and pushing the snow aside so they were relatively inactive in the cabs of these vehicles The cabs were unheated We found that the men could run a machine for about an hour At the airstrip there was a Wannigan with coffee and fire a man would go in there and then someone else would come out and run the vehicle They spelled each other In a 12 hour

day they actually put in 6 hours of work. They could hardly stay outside longer than that.

Behnke. How many men shaved?

Taylor. About 50 per cent. The percentage went up and down. There was enthusiasm for growing beards at first but this passed. Then when the summer folk were expected they grew more beards.

Barquist. Will you tell about the merits of beards versus no beards at −50 F temperature?

Taylor. This is a personal matter. The men who had beards did not need to cover their faces; they were quite comfortable with the protection the beard afforded. Those who were clean shaven generally wrapped the lower part of the face with a scarf; either that or took the hood and buttoned it to make a sort of tunnel to give them a vestibule in which to warm the air before it got to the face.

Blair. I wonder how much of the value of the beard is really psychological. We had a rather amusing but valuable experiment at Churchill in which a Canadian medical officer grew a beard on only half of his face; the other side was kept clean shaven during the winter. We put thermocouples on either side of the face. Though he said the bearded side of his face was much warmer and more comfortable the thermocouples showed the same skin temperatures on both sides out in the cold and the wind. We felt that maybe a lot of the benefit was that it gave him a sense of protection, more fortitude to go out in the cold and less anxiety.

Burton. I think I am unique in having some calorimetric measurements on heat loss from human heads with beards and without beards. For the centennial of our city a "Hairy Apes" Club had been organized by a local business. I was developing head-calorimetry and induced ten of these men to be subjects. The results were that the extra insulation of the beard was hardly measurable. The fact is that the beard grows on the coldest part of the face which is losing a small proportion of the total heat.

I have no doubt that the areas on which the beard grows are the places most susceptible to frostbite. So apart from the icing-up problem with beards I would think that they are of some help.

Irving. Did you test the temperature of the skin under the beard and unbearded surfaces?

Burton. We stuck thermocouples in the beard and compared with afterward. Under the beard the skin temperature went up. This also made the beard rather ineffective as total insulation because there were correspondingly higher skin temperatures underneath which tended to increase the heat loss.

thick one If they do start one they shave it off quickly to avoid being teased

Behnke Was there a tendency for the men to get stale and did you find that they had difficulty in coordination? Was there evidence of deterioration and increased accident proneness?

Taylor I think so I think our productivity went down as a function of fatigue and really loss of enthusiasm The men were terribly beat as the expression goes by the end of the second summer before we went on board the ship It came to be for all of us simply a matter of doing the job somehow or other from day to day to get through so we could get on the ship and come home

Behnke You were counting days?

Taylor Yes we definitely were doing that

Talbott During your first night was there a peak period of activity or of efficiency rather?

Taylor I think our peak of efficiency occurred before it got dark In the early stages when we were living in tents and building the winter quarters we really did a tremendous amount of work Then as that work was completed and as darkness came on we were able to let up a little bit

Around 1 July when we actually first realized the magnitude of the job of building the runway we had another little spurt but it didn t last very long We seemed to be dragging pretty much by 1 October Then the summer was for most of us just a grueling experience of long hours and hard work and wanting to go home

Behnke How many would volunteer again for the experience? Is it once in a lifetime?

Taylor I don t think so Of course when we left we said Never again but the 15 odd months since we departed have softened the memory for me at any rate As I communicate with my fellows who were there they too have forgotten some of the emotional overtones of how it was One begins to wonder again what it looks like there Dr Siple has done this repeatedly Maybe he would make a remark about it

Siple This habit of forgetting the bad things is characteristic Many men who say they never would go again are ready to go again 6 months later Out of our eighteen men at the Pole we had three who volunteered to stay on but were not permitted to do so One of them wrote me recently that he expects to go back next year He happened to be one of the three who wanted very badly to stay*

*[illegible]

Blair As to the actual temperature on the bare face and bearded face, we could see practically no difference, although the test subject for this study said the bearded side of his face definitely felt warmer

Taylor Several of our men shaved in mid winter When they had to work outside, by preference, they grew beards again and by that time, we were at a stage where we weren't showing off before each other They found it much more comfortable I don't recall any frostbite at all under a beard, but we occasionally had touches of frostbite on exposed skin

Hock Did the beards ever ice up?

Taylor They iced up, although the skin apparently was still comfortable under the beard

Hock Icing up might take more than an hour

Taylor Our men stayed out more than an hour We made ice reconnaissances, for example, from time to time. We would be out all day, generally working quite hard, walking fast and hauling a sledge

Blair Probably icing was primarily a problem of a long beard or a short beard The icing on the short beard will go down to the skin and produce maximum cooling

Siple At −60°F over a period of 2 or 3 hours, a pretty solid cake of ice gets built up on the beard Sometimes it gets frozen to one's clothing, and it takes minutes to get loose after coming indoors

Irving It gets caught in the zipper, too

Siple That is possible, too, when it gets long and cold enough We found, actually, that a beard is extremely messy and undesirable on the trail, where one is living out of doors and cannot thaw the beard out until getting into a sleeping bag At the Pole, those of us who frequently got our beards very iced up, would go indoors and draw a basin of warm water, put our face down in it, and thaw the ice out quickly This made a beard more tolerable because otherwise we didn't like the long period it took to get the ice melted out of the beard upon coming indoors after exposure to really low temperatures One of our men shaved half his beard off, and he went around for a day or so, trying it as an experiment. He felt a definite difference

There was no question of feeling a decided difference on your face after shaving off a year old beard In fact, one feels an almost shocking cold for a while, right after shaving There is also a considerable difference between the ability of men to grow beards too Some men do not grow them simply because they can't grow a good

thick one If they do start one they shave it off quickly to avoid being teased

* *Behnke* Was there a tendency for the men to get stale and did you find that they had difficulty in coordination? Was there evidence of deterioration and increased accident proneness?

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Around 1 July when we actually first realized the magnitude of the job of building the runway we had another little spurt but it didn t last very long We seemed to be dragging pretty much by October Then the summer was for most of us just a grueling experience of long hours and hard work and wanting to go home

Behnke How many would volunteer again for the experience it once in a lifetime?

Taylor I don t think so Of course when we left we said Never again but the 15 odd months since we departed have softened the memory for me at any rate As I communicate with my fellows who were there they too have forgotten some of the emotional overtones of how it was One begins to wonder again what it looks like there Dr Siple has done this repeatedly Maybe he would make a remark about it

Siple This habit of forgetting the bad things is characteristic Many men who say they never would go again are ready to go again 6 months later Out of our eighteen men at the Pole we had three who volunteered to stay on but were not permitted to do so One of them wrote me recently that he expects to go back next year He happened to be one of the three who wanted very badly to stay *

If a man has had a reasonably happy experience, he usually wants to go back again

Burch What is the expected life of the buildings?

Taylor I don t know Do you know, Dr Siple?

Siple It depends on factors such as whether they are occupied or what the snow loads on them were We estimate about 10 years life for the Pole Station buildings, and, because McMurdo does not get covered over, those buildings might possibly last 15 or 20 years

Behnke Are they fireproof?

Taylor They aren't fireproof

Behnke Isn't that a danger?

Taylor That is a danger We had a fire watch, of course, and fire alarms in all the buildings, but actually had no fires The climate is so dry that everything burns with great speed The problem of ventilation in buildings is related to this, perhaps Since we weren't covered with snow, we ventilated the building through one end with electric fans and in this way kept a good bit of air moving We also had carbon monoxide alarms In the garage and other places where carbon monoxide really was apt to develop, the men would turn off the alarm because it was always ringing It had to be turned on about once a day Our metal workers had a shack in which they worked, which was very poorly ventilated I saw a good deal of metal fume fever, zinc fever

This problem of ventilation in spaces in which there are noxious fumes is a real one The tendency, of course, is to close down all the ventilation to stay warm We finally made the men ventilate the garage and the metal workers' shop, so they were working under quite favorable conditions

Behnke Have you any comments on food? The problem of food supplies is a very important one

Taylor We had very good food Through the winter there was fresh meat which was frozen and stored outside A good deal of fresh frozen vegetables had been brought down Even in the Antarctic the preservation of frozen foods presents a problem, because it does become warm enough in the summer to spoil the frozen provisions and refrigerators are needed In general, we had a varied menu

I believe that we were provided with 6000 calories per day per man, and I think we probably used almost that amount of food We also gained weight on that amount Even with hard work outdoors our men gained weight I don t have actual figures on how much was gained but it was considerable

In the summer when we had a huge influx of visitors the food

supply got rather short and we were using mostly canned goods The amount of food was not critically low but the variety diminished considerably

Behnke Could you have lived on canned food?

Taylor I think so I don t know we didn t have to

Siple We did in the past

Taylor That is true I set out a dish of vitamins in the mess hall and invited the men to take them as a supplement if they wished Some did and some didn t There was no difference in the health of those who did and those who didn t as far as I could see

Behnke Not with 6000 calories a day!

Burton To return to the matter of fire hazard I don t think it is properly appreciated that the fire hazard can be greater at 10 000 feet than at sea level People tend to think that since the oxygen tension is lower the fire hazard might be less but the contrary is true

Taylor How is that?

Burton The reason is that the burning of many substances depends not upon the oxygen tension but upon the competition of the oxygen and the nitrogen molecules to reach the flame *i e* on the percentage of oxygen which is unchanged at altitude Tapers or candles burn almost twice as fast at 10 000 feet During the war there were some fires in experimental chambers kept at 10 000 feet which illustrated the fact that the fire hazard is very great The reason is the decreased heat loss at altitude which raises the temperature of the flame

I wondered if Dr Siple had any observations on this I think the opposite point of view is very dangerous People do not realize the hazard is greater at high altitude than at sea level

Pace This is pure oxygen and not in air

Burton This is in air

Pace I have to disagree on the basis of the simple observation that a cigarette lighter at 10 000 feet just doesn t burn as vigorously as at sea level

Burton This may have something to do with the igniting temperature of the fuel The experiment has been done by burning a candle I smoked cigarettes in the chamber and they burned faster than at atmospheric pressure

Talbott It may be true in the chamber but not at high altitudes

Burton As to measurements of rate of burning I am not speaking from my own observation Dr Drinker pointed this out

Talbott That was in a chamber?

Burton They were all chamber experiments

Behnke At pressures above one atmosphere, the cigarette burns much faster Under these conditions the fire hazard is great

Burton The real difference at altitude is that the heat loss is retarded by the thin air, and therefore the flame is at a higher temperature Anything which is burning depends upon vaporization and vaporization goes up logarithmically with the temperature of the flame This is the reason a taper, or a candle, burns so much faster at altitude

I think it is because the heat loss is very much retarded The insulation of the air at 10 000 feet is only about half what it is at ground level This means the flame of the candle has to get a good deal hotter Therefore, the vaporization is very much faster

Fremont Smith It is interesting that we have actually contradictory evidence on this purely physical problem by competent observers on both sides I hope that someone can obtain data on it

Reynolds This was worked on, again in chambers, in the National Institutes of Health a number of years ago, where actual measurements for maintenance of flame were made The substance being burned is an important factor in this, so the candle is a particularly good item to demonstrate Dr Burton's point, as in this case one wants to maintain temperature over a wide enough area to get wax in a wet enough condition to be sucked up by capillary attraction in a wick however this is not true of the other situation The work at NIH was done with wood, wax, and hydrogen and they all burned differently under low pressure

Burton There wouldn t be this critical factor of vapor in the burning of curtains This is a case, like the candle, where the temperature of the curtain the rate of vaporization, is very important

Irving I had heard it said that people confined in close quarters could so diminish the oxygen in the air as to cause the extinction of pipe cigarettes and candles

It didn t sound quite right to me until in burning candles in a tent impervious to wind we observed the candles go out and even the stoves cease to function although we, ourselves, had not yet experienced any respiratory difficulty Upon analysis of the air, the extinction of that particular candle happened to occur at about 16 per cent oxygen That is when only about 5 per cent of the oxygen had been removed (1)

Then thinking it might be a matter of pressure of the air to the same degree, we found the candle still burned at that oxygen pressure in the air So there is certainly something in what Dr Burton said

Burton: We did a similar experiment on the roof of the Banting Institute in Toronto. Three of us sat in there and closed up the outlet the way one would in the Antarctic and heated some soup over a Sterno can. The oxygen went down to 17 per cent and the candle wouldn't burn any more. Incidentally, I got quite a case of carbon monoxide poisoning from this experiment. On the other hand, in the old analogy by which we indoctrinate the Air Force we say, "If you take a candle up in a chamber it goes out"; this is not true. The candle is still burning at 40,000 feet. So although it is a beautiful way of getting across the need for oxygen, it is completely false scientifically. A candle depends upon the oxygen percentage, not the tension, and 17 per cent is sufficiently low to make a candle flame go out.

Pace: In a recent paper on this subject, Simons and Archibald (—) at Holloman Air Base actually measured burning rate of paper at various altitudes and various partial pressures of oxygen. The point was made very clearly that burning rate depends on both oxygen partial pressure and nitrogen partial pressure, so your point about the competition of the nitrogen is quite correct. But in atmospheric air at various altitudes there is still the same proportion of oxygen to nitrogen and burning rate is inversely proportional to altitude.

Burton: At altitude the temperature of burning becomes much higher because the heat loss is impeded. Therefore, to get the heat balance there is a higher source temperature. In many substances, particularly waxy ones, this enormously accelerates the vaporization of the fuel.

Siple: The little PX at the Pole was supplied with quite a number of catalytic hand warmers. The men wanted them because they thought they would be useful. We experimented day after day trying to get the warmers lit and keep them lit. Using the prescribed fuel and precise instructions, they could be started under warm conditions and they would keep going. However, if they became a little cold they would go out and starting them cool at high altitude was almost an impossibility. One out of ten times one could be started but it would go out again in a few minutes. We had some of the same difficulties with cigarette lighters.

Taylor: The clothes we had were excellent. I haven't said anything about cold injury because we didn't have any to speak of. I think that speaks extremely well of the progress which has been made in housing and clothing over the course of the last few years. We had just occasional cases of frostbite of the face where a man

turned his face to the wind unknowingly, or of the hands where a mechanic had to remove his gloves in order to do a little job requiring fine manipulation That was the extent of it

We had a little snow blindness in the summer mostly in people just entering the camp who didn t yet realize the importance of protecting the eyes against the radiation

Lyman What kind of glasses were used for protection against snow blindness?

Taylor Standard Air Force issue goggles Capt Hedblom the Task Force Medical Officer carried out an extensive comparison of different kinds of glasses I don t know what his conclusions were We generally used the dark aviator type goggles

Lyman Just dark brown?

Taylor They were dark brown with about 10 per cent light transmission I didn t have the data on the glasses but as I tested it with my light meter they came out about 10 per cent The glasses are just a standard part of one s costume They are put on with the hat

The men who were to go to the Pole Station were selected on the basis of their performance during the winter I think Dr Siple will attest to the fact that they really did an excellent job of building the Pole Station base in a shorter time than had been anticipated

As he said one man had what was clearly altitude sickness and was incapacitated for a few days Another man developed a cough and pleurisy which was painful but he didn t have to come out He stayed on the job there

The men reported it took about 2 weeks to become accustomed to the altitude and be able to do a good day s work When they came back to McMurdo Sound however they all commented on how pleasant it was to take a breath of air you could really get your teeth in or your alveoli around or something like that

I haven t said anything at all about the operations of the second summer but we were very busy in the camp There was a great deal of air activity

Our severest test in the medical department came in October 1956 when a plane crashed We had four fatalities one man slightly injured and three men very severely injured We kept them in camp for about 4 days until they could be evacuated to New Zealand

Two of the principal problems to which we did not have satisfactory solutions were the transportation in the cold of injured personnel and the administration of first aid at the site of injury to severely injured personnel At the time of the plane crash the temperature was −20 F We had a Wannigan at the airstrip near the

site of the accident but it was not large enough to accommodate all the injured men. We were forced to put them in sleeping bags on a sled and bring them back into camp behind a tractor. This took about an hour and the delay contributed, we thought, to the death of one of the men.

Fremont-Smith: Of shock?

Taylor: He died of hemorrhage. We tried to administer dextran at the airstrip but, of course, this was impossible at −20°F. This problem is one which certainly deserves attention.

In any kind of military operation involving actual combat in cold climates, of course, the problem of first aid at the scene of injury would be a tremendously important one and, as far as I am concerned, is really deserving of a great deal of careful study, if such is not under way.

Our experience with respiratory disease was quite comparable, I think, with what Dr. Siple has reported. As I said earlier, the men who came into the camp in the summer had just as severe colds as those who had spent the winter there and, as far as I could tell, suffered even more. Our respiratory passages had adapted themselves to the dryness of the air, but this was a great complaint with those who had just come in in the summer. When this was complicated by upper respiratory infection, it made the men quite uncomfortable.

We did not have much in the way of bacterial complication of our respiratory infection, though we used some antibiotics whenever there was exudative pharyngitis or fever of more than 102°F. Most of the men simply had coryza or evidence of bronchitis and were not febrile.

I want to say a little about the pursuit of research activities in isolated stations such as the one at McMurdo Sound. During the winter I wasn't really very busy most of the time and I tried to set up some experiments in which I was interested: temperature coefficient of oxygen consumption of the lichens which grew in the melt ponds around the camp. I finally gave it up. It seemed to me that whenever I got the equipment out and started to work on it I was sure to induce a fractured leg or something.

The men who relieved Dr. Ehrlich and me and the doctors who were sent to some of the new stations in the Antarctic were asked to do a good deal of medical investigation during their period of isolation. I thought this a rather poorly conceived scheme because, if ninety-three men are engaged in strenuous physical activity, as ours were, there will be enough in the way of accidents to keep the attention of the medical officer on clinical problems rather than on

a research problem I think it would be very difficult to do any but the simplest research along with clinical responsibilities Last year the Air Force sent a physiologist to Little America to pursue a specific research program

The over all accomplishment I think from the medical and physiological point of view at McMurdo Sound is noteworthy in that we put ashore a party of very inexperienced people and we were able with modern techniques and technology to build a camp and live there the largest party ever to have wintered in the Antarctic quite comfortably and reasonably happily with a minimum of trauma and virtually no cold injury

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PHYSIOLOGICAL STUDIES IN THE ANTARCTIC*

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THE STUDIES THAT WE ATTEMPTED to carry out were really aimed at making a very general assessment of a diffuse condition that has gone under a large number of names so I think it would be best to discuss the nomenclature at this time We were studying the degree of stress that may or may not have been present in Operation Deepfreeze I personnel I would like to define what I call stress and then show how we have evolved a technique of trying to assess it Figure 37 shows some very basic physiological principles It is highly schematic and it brings out some of the problems of trying to estimate the performance and effectiveness of human beings in environmental or other situations

A major difficulty is indicated by the dotted lines at the top of the graph labeled performance These represent generally speaking performance tests of one kind or another that the psychologists particularly apply in attempting to assess the degree of fatigue or lack of ability to perform in a situation Usually they show the characteristics indicated in Figure 37 Not only do performance tests often show a very small amount of decrement until the stressor involved in a situation becomes extreme but in addition there may occur the phenomenon called facilitation When individuals are placed under stress performance of a given task sometimes is actually improved

Horvath Is this an indication of failure to give a good adequate definition of normal?

Pace No I don t think so I think there are many instances in

* The material upon which the following discussion is based was obtained with the support of contracts between the Office of Naval Research and the University of California and between the Arctic Institute of North America and the University of California

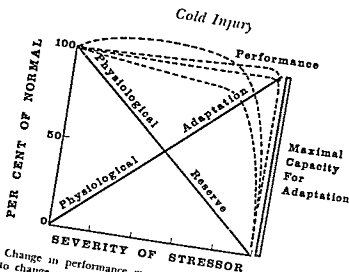


FIGURE 37 Change in performance measures as a function of severity of a stressor compared to change in degree of physiological adaptation and decrement in physiological reserve

which facilitation has been demonstrated using fairly simple tests for example, in the measurement of such factors as simple reaction time, complex reaction time, or the ability to do arithmetic problems

Henschel What is the environment under which these tests are carried out? That will influence the outcome very much You must also define the test situation very exactly

Pace Yes, I have avoided that for the moment

Henschel This increase may mean only a better environment under which the work is being done

Horvath Under some circumstances

Pace If you include the psyche, admittedly This gets into the problem of motivation

Henschel Even physical environment

Keller You can demonstrate whether it is true facilitation

Pace The psychologists call this "facilitation" My point is that performance tests are not well suited for measuring what we are after At the same time, when an organism is placed in an unfavorable environment, we all talk about the adaptation that the organism is called upon to make It seems reasonable to assume, therefore, that for at least some physiological functions there might be a more direct correlation between the degree of adaptation and the severity of the stressor than there is between performance and the severity of the stressor

A simple example of this is the direct correlation between in

crease in circulating hemoglobin and the altitude above sea level to which an individual acclimates. On the other hand there must be a limit to the degree to which an organism can adapt. Thus as the individual approaches this limit the degree of reserve upon which he can call is correspondingly reduced. In order to estimate the amount of adaptation or compensation an individual has made it is necessary to push him to his limits so as to measure the amount of reserve that remains. This is a trick that has been resorted to frequently by physiologists for example in such procedures as exercise tests.

The problem then is this and I am sure that most of us have considered it at one time or another. At the same time that the adaptation occurs there must be underlying metabolic changes going on in the organism to allow the adaptation to be made. This is where the endocrinologists very broadly speaking have made a major contribution in the recognition of the importance of the adrenal cortex as a factor in the general mechanisms whereby the organism seems somehow better to achieve adaptation to stressful situations.

Clearly there are other endocrine systems that are involved. I think it is being recognized more and more that the thyroid for instance is another part of this adaptation phenomenon. In any case it would seem logical that by measuring changes in metabolic entities which are under the influence of the adrenal cortex we could arrive at some kind of direct correlation similar to that shown schematically in Figure 37 labeled physiological adaptation.

Thus what we can do is measure various metabolic constituents of the blood and urine of the individual which we know are affected in one way or another by secretions from the adrenal cortex and perhaps utilize these as a scale whereby we can estimate the degree of adaptation that the organism is called upon to make.

Hart Does this assume a direct correlation between the functional changes in the endocrine glands and the adaptive changes in the whole organism?

Pace One can make this assumption but on the other hand I don't think it necessarily follows for any single metabolite. Actually what we have been attempting to do is to answer that kind of question. What I want to do here is not to get into an elaborate detailed analysis of the reasoning that we have been going through but rather to give examples of how we have attempted to utilize such a technique to estimate the severity of three situations where individuals have been exposed to cold environments so as to make some

kind of quantitative comparison of the severity of these three situ-
ations How successful we have been remains to be seen, and I am
curious to see what the response of the group is to this
One other point before getting into the actual data is shown by
Figure 38 which is another gross oversimplification but once again
is intended to emphasize a principle If one considers that these
underlying biochemical changes may be going on one can use the
sort of analogy shown In the normal' situation we have the kind
of thing that is represented by the first container on the left, where a
given amount of a metabolite may be regarded as being produced
by the organism in the course of the normal intermediary metabo-
lism and is leaking out so that a constant blood level is maintained
We can assume that under the influence of a stimulus or stres-
sor the organism might be called upon to make more of a given
substance, and is at the same time faced with maintaining homeo-
kinesis Thus we would tend to see the kind of situation shown in
the center compartment, where if we simply measured the level of
this material in the blood we might not necessarily expect to see
much change However if we now measure the turnover of this
material—and one way of doing this is by examining the urinary ex-
cretion rates rather than simply the blood level—we get a little better
idea of the rate of turnover that the organism is effecting We can
now say that this organism is in a state of stress We don t necessarily
see any change in the blood level but the drain valve of adapta-

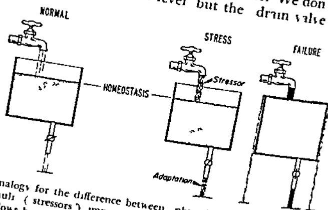


FIGURE 38 Analogy for the difference between physiological stress and physiolog-
failure Stimuli (stressors) impinging upon the organism may be increased b-
adaptation allows homeokinesis to be maintained as represented by greater opening
the drain valve Degree of stress is represented by the higher flux density within th-
system Failure occurs when the adaptive mechanisms are exceeded and homeokines-
is lost *

*In Figure 38 for homeostasis read homeokinesis

tion is opened more widely than it was when the stressor was not being applied so that homeokinesis is maintained Finally we reach the point represented by the last compartment where the stressor exceeds the capacity of the adaptive mechanism and loss of homeokinesis or failure ensues It bears emphasis that in this analogy stress and failure are clearly different Another way of putting it is that stress may be regarded as the physiological cost to the organism of maintaining homeokinesis

Figure 39 shows the complexity of the interrelationships we are discussing In the upper right corner are two boxes labeled environmental stressors and psychiatric stressors I distinguish between these because this problem invariably arises How do we distinguish or is there a difference between the physiological responses that occur in a business man who is brooding over imminent bankruptcy and the responses that are seen in an individual exposed to a physical stressor—let us say exposure to a cold environment? I think the answer is Certainly there are differences because different receptors

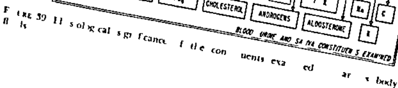


Figure 39 Physiological significance of the constituents examined in various body fluids

are involved but there are also undoubtedly common pathways that are shared by the responses even though the responses are triggered by different receptors This I believe without belaboring the point *is now generally accepted The concept of a pathway from the* central nervous system through the hypothalamus to the pituitary and the ultimate response of the adrenal cortex has been voiced by many physiologists Here we see an example of how different kinds of stimuli can lead to a common effector—in this case the adrenal cortex

The cortex not only puts out substances in the form of adrenocortical hormones indicated in Figure 39 as the glycotropic factors cortisone and hydrocortisone plus the androgens and aldosterone but these compounds in turn result in metabolic changes of one kind and another The mechanism is far from understood at the present time but it has been clearly established that there is some kind of correlation between changes in the level of circulating hormone and particular metabolites

A number of these are shown in the box at the bottom of Figure 39 which is outlined by the double line For example there are changes in electrolyte levels which seem to correlate with changes *in aldosterone level There are changes in glucose level* phosphate uric acid cholesterol and the other constituents shown that at one time or another have been demonstrated to be affected by the level of one or another of the hormones indicated

The principles upon which our studies have been based is a simple one If we wish to find out something about the relationship between various stressors and the adaptation that the organism makes the assumption is that by simply measuring the body fluid constituents listed in the double lined box at the bottom of Figure 39 plus whichever of the hormones that can be measured *directly with the* means readily available it is possible to arrive at some kind of estimate of the amount of adaptation that the organism is called upon to make in a given situation This we have done

Again by way of background—not that I want to get into a discussion of the effect of combat at this time—the data from a combat situation that we studied (1) bring out a number of these points more effectively than some of the other environmental situations with which we have been concerned

I would like to present the data on the physiological effects of combat on infantrymen in Korea as though they were the result of a logical development but they are not We had decided that one of the interesting things to study would be the adrenal cortical response

in such individuals and as we did not have much time for preparation we went ahead with the work I would now like to show some of the data we obtained because the more we worked the more we realized that perhaps this was a way of approaching many environmental situations We now have quite ambitious plans for measuring not only as many metabolites but also as many hormones as possible simultaneously in the same individual The more one thinks about it the more one realizes that here are the really controlling substances This makes sense from very elementary physiological considerations With a given number of receptors it is easy to see that for different environmental situations these various receptors are going to be stimulated to differing degrees depending on the situation Consequently it is not difficult then to go one step further and say that these pathways will result in a response pattern that is the summation of the particular pattern of receptors that have been stimulated

Fremont Smith It may be the product and not the summation?

Pace Whatever the integration yes So one should not be too surprised to see a different and characteristic metabolic pattern for each environmental situation in which an organism finds itself

I like to think that if we get the important parameters in this system metabolically—and this is why the hormones are a logical point of first attack—it will then be possible to sort out the physiological response to a particular environmental situation and try to analyze the integrated response of the entire animal in terms of the individual mechanisms that may or may not be brought into play

What we have done so far is only a very crude beginning I hope th

an

be looked at as a pattern one should not try to make sense out of each individual component It is quite typical of data one obtains when the blood constituents indicated are measured in many stress situations

In the upper third of Figure 40 are the data from two groups of normal healthy soldiers One group was measured in Japan completely out of the combat area All of these data were obtained in 1952 while the Korean War was going on The other data in the top portion of the figure are from a group of front line infantryme taken in what the Army called a quiet sector of the MLR (mi line of resistance) facing Chinese Communist enemy troops Tl Army defines a quiet sector as one where not much is happenin There aren t more than occasional mortar rounds coming in Tl

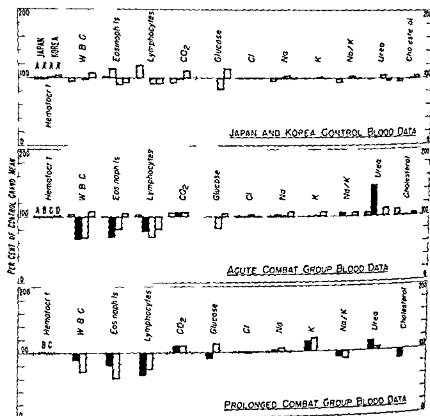


FIGURE 40 Mean blood constituents of the control and combat groups expressed as a percentage of control grand mean for each constituent Upper section represents test data A and retest data A′ obtained several days later for Japan and Korea control groups Center section represents data from acute combat group obtained 12 hours before combat A 12 hours after combat B 5 days after combat C 22 days after combat D Lower section represents data from prolonged combat group obtained 12 hours after combat B 10 days after combat C Dashed lines represent standard error of control grand mean for each blood constituent measured Reprinted by permission from Pace N Schaffer F L Elmadjian F *et al* Physiological studies on infantrymen in combat *Univ California Pub Physiol* 10 1 (1956)

men engage in such routine duties as ambush patrols at frequent intervals Of course, people get killed once in a while, but I found that even the infantrymen themselves regard this as not too bad a situation

Comparing the metabolic data from infantrymen in Japan who were completely away from the psychological problem of imminent danger of combat with data from soldiers in this quiet situation in a combat area—living in bunkers, eating food brought up in jeeps

under occasional fire and so on—we found no significant difference between the two groups or from the normal population as a whole. Furthermore, we assumed that the soldiers in the quiet sector constituted a reasonable control for the two situations we were interested in actually examining.

The feature that characterizes Figure 40, in spite of all the little blips, and particularly as will be shown in Figure 41, is the lack of variation relative to the normal population. What we have plotted is the per cent change of each constituent from the normal value, calling the normal value 100 per cent. The actual values for normal were taken from the general literature, from such sources as the Handbook of Biological Data. Thus, for each of the groups examined, we expressed the mean value for the group as a per cent of the mean value for the general population. The upper third of Figure 40 shows that the control groups we examined did not differ significantly from the normal population in either case.

Taylor: What are the limits? Are the dotted lines of any significance?

Pace: Yes, they indicate probable error of the mean. The middle portion of Figure 40 shows what we found in the blood of a group that was called upon to carry out an intensive combat action which we termed the Acute Combat group. They were asked to occupy a small hilltop in front of our fixed lines, and it had been anticipated that they would encounter relatively little resistance. It turned out that the Chinese Communist forces had set this up as a trap. The infantry company of about 150 men that was to carry out the job was unexpectedly beaten very badly and suffered some 61 per cent casualties in about 18 hours of very intensive action. There is no question that these individuals were under stress.

On the whole, we saw little change in the blood of these men as shown in Figure 40. A few factors did change. For example, we saw a definite decrease in the number of circulating leukocytes. There as a substantial and statistically significant increase of the plasma en level, together with a possibly significant drop in plasma glucose, but that was about all.

Henschel: How long after the cessation of hostilities did you get the samples?

Pace: Approximately 12 hours.

Henschel: All about the same time?

Pace: Yes.

Taylor: How many subjects were there?

Pace: Twenty.

Taylor What is the significance of the different shadings? For example under white count you have four different shadings?

Pace The first one was the day before the action the second darkest one is immediately after combat the third one is 5 days after combat and the fourth is 22 days after

Keller What about the variability? For instance taking eosinophils didn t you have a 100 per cent eosinopenia in some cases?

Pace No we did not see 100 per cent We have only seen that in one situation which I will come to a little bit later

Keller That is not a very significant drop

Pace The eosinophil drop was right on the border of statistical significance (P $<$ 0 05) I will show the constituents that changed significantly in a summary (Figure 47 see page 167) later The main point I am trying to get across now is the order of magnitude of the changes more than anything else

Burch Was there dietary control?

Pace We made our control measurements in soldiers eating the same things living under identical conditions with the difference of actual combat

Burch I was thinking about the urea for example Did they eat a big heavy meal when they returned?

Pace No We got them before they even had a chance to take a bath or do anything else

Bass Were they dehydrated?

Pace Yes somewhat I think Figure 41 shows it

Carlson Are the four bars correlated horizontally to the time of day?

Pace Yes This is one thing about which one must be very careful

Bass Did the same twenty men get through the action without casualty? You say this represents twenty men?

Pace Only five of our original twenty subjects came back so we had to take fifteen more out of the same group Then to check this we used the 22 day recovery figures as a comparison with the other controls to insure that we hadn t made a mistake in selecting the fifteen others In the comparison between the five that we had original data on and the fifteen we obtained additionally there was no difference

Figure 42 shows the order of magnitude of the diurnal variation to be expected in some of the constituents we examined As is evident it is substantial and one does have to be careful to control time of day This is a very important factor We also have found that diet certainly produces variables but they are relatively minor

in comparison to the changes that we are interested in examining here

Burch Electrolytes too?

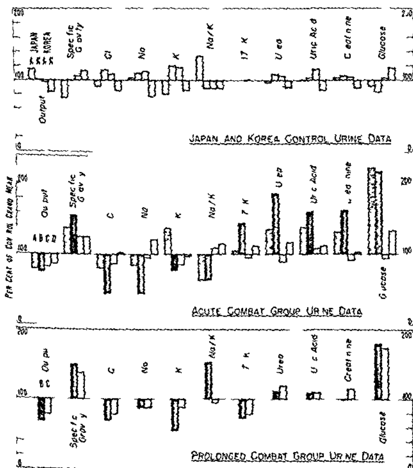


FIGURE 41 Mean ur ars n t tuents f the con rol and combat gr ps expresse l as a perce tage of co tr l gran l m an f r ea h const tue t Upper sect rep resents test data A an l retest lata A obta ned se eral days later for Japan a nd Korea contro l gr ups Ce ter sect n represents la a f m A e Com ba gro p ol ta ed 1? hours before co bat A 1? ho rs af er con l at B da s after c mba C 9 la s after con bat D Lower se t rep rese ts la a fro n Pr longed Combat gr up ol ta d 1? h rs af er combat B 10 la s af er comba C Dashed l es rep esent standar l errors of c tr l grand mean for each nary c tuent meas red Repr nted b permission fro la e N Schaff r F J Fin a l ja l et al U g al stu l es on (ancer men n ba t w t nl fo P l 11 ol 10 1 1956

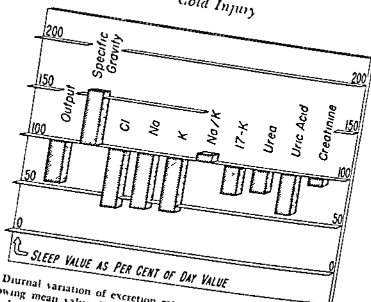


FIGURE 42 Diurnal variation of excretion rate of urinary constituents in normal individuals showing mean value during night sleep period expressed as a percentage of mean value during day waking hours Reprinted by permission from Pace N Schaffer F L Elmadjian F et al Physiological studies on infantrymen in combat Univ California Pub Physiol 10 1 (1956)

Pace Not so for the electrolytes themselves but changes in sodium potassium ratio occasionally exceed what would be expected, except in extreme dietary changes

The bottom portion of Figure 40 shows the data obtained from another infantry group that engaged in combat for 5 days in exactly the same location, which we termed the Prolonged Combat group They went in a day later than the Acute Combat group and successfully occupied the hill, sustaining something like 25 per cent casualties in the 5 day period Again, we examined them shortly after they came out of the active combat area

Keeping in mind the scale of the changes in the blood on Figure 41, one can see the kind of thing that we found in the urine These are excretion rates for the individual constituents plotted as a percentage of normal excretion rate in each case This is quite different from the blood There is a very marked difference between the two combat groups and the controls, whereas the two controls are quite reproducible within themselves, by and large

We did measure 17 hydroxycorticoids in some of these individuals But unfortunately, in preparing the samples, a great many

of the determinations were spoiled so that a statistical evaluation could not be made

Keller Do you have any evaluating data on exercise on activity during the time involved?

Pace The men were not unusually active The job they had to do consisted of going up a relatively small hill Once they got to the top they were quite well pinned down most of the time they were there They engaged in occasional hand to hand actions but these (I think there were two) lasted only a period of minutes at a time Most of the time they were there they just tried to get what cover they could

Keller Being without food was not a factor?

Pace They weren t out that long a period of time

To continue the data for the Acute Combat group really look like an exact duplicate of what one would see if one injected a substantial dose of ACTH into normal individuals The Acute Combat group showed perhaps what one might have predicted—a massive response of the adrenal cortex to this rather intensive stressor

There is some indication of dehydration in that the urine output went down slightly from the control value obtained the day before I think they were also somewhat dehydrated the day before but there was not too much change as you can see here Likewise the specific gravity went up slightly correspondingly but altogether it was evident that major dehydration was not present

Taylor Over what period was the urine collected?

Pace A 4 hour period

Taylor Was the blood taken at the beginning of this 4 hour period?

Pace No at the end Again as I say I don t want to get into these combat data too deeply I am introducing them primarily to establish orders of magnitude of change I would like to talk more about the cold data that we obtained

Figure 43 summarizes the differences that we saw between the group that was in the combat situation for a short period of time as compared to the group that was in for a more prolonged period There are some very interesting differences between the two Primarily the intriguing thing to us was the fact that the 17 ketosteroid picture was just reversed as was the sodium potassium ratio To repeat what I said earlier the data from the Acute Combat group are like what one would expect from giving a massive dose of ACTH to normal individuals

My remaining figures (Figures 43 to 47) show that with a very few exceptions the 17 ketosteroid excretion rate parallels quite

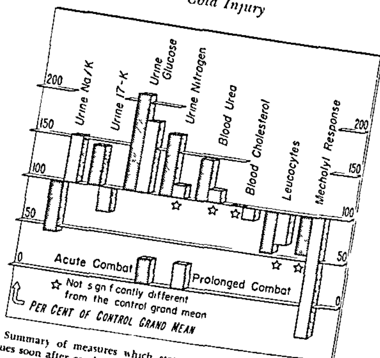


FIGURE 43 Summary of measures which statistically were significantly different from control values soon after combat in either of both acute and prolonged combat groups Values are expressed as a percentage of control grand mean for each measure Reprinted by permission from Pace N Schaffer F L Elmadjian F, et al *Physiological studies on infantrymen in combat* *Univ California Pub Physiol* 10 *1 (1956)*

closely the 17 hydroxycorticoid excretion, but that the latter is much higher Thus while the 17 hydroxycorticoids apparently constitute a more sensitive measure than the 17 ketosteroids, I think it is valid to use the 17 ketosteroid excretion rate, although 17 ketosteroids aren t the clean index that the 17 hydroxycorticoids represent

In any case, Figure 43 shows two situations where individuals exhibit quite different metabolic patterns

Bass What is "mecholyl response"?

Pace This was a test we devised to make an estimate of sympathetic reactivity, for want of a better term This was at Gelhorn s suggestion * It is similar in many respects to what Funkenstein has been doing at Harvard (2) It consists very simply of injecting mecholyl and then measuring the blood pressure The immediate

*Gelhorn Ernst Dept of Physiology University of Minnesota Minneapolis Minn Personal Communication

drop in pressure is followed apparently by a reflex sympathetic response. There is characteristically an overshoot until the insult to the sympathetic nervous system, if you like, is over with.

Davis: What do you take as your response?

Pace: The measurement of the area under the overshoot of the curve.

Davis: You neglect the fall?

Pace: Yes, the fall is caused by the mecholyl. We have assumed that the sympathetic nervous system is thereby stimulated. So by measuring the area under the curve—the area of the overshoot—one has a measure of reactivity of the sympathetic nervous system to the stimulus produced by the mecholyl induced blood pressure drop.

Davis: Would you get exactly the opposite if you injected epinephrine?

Pace: Yes, this was done. In fact, that is what Funkenstein (2) does. He uses epinephrine and mecholyl and compares the two responses. We were concerned only with the mecholyl response. In the Prolonged Combat group, the response was significantly reduced. The reason the value shown in Figure 43 goes below zero is that in this group we saw no overshoot and actually the curve did not come back to the pre injection level for a very considerable period of time. So we had a negative overshoot, if you like. We interpreted this as a marked depression of the sympathetic response in these individuals. It was very interesting.

Keller: Might that be interpreted as the prolongation of the action of the mecholyl?

Pace: It is possible, but I don't think so.

Keller: You would be measuring something new, something different?

Carlson: It would be sensitivity to mecholyl, wouldn't it, in the Funkenstein test?

Keller: It could be.

Fremont-Smith: You have a rate of destruction, perhaps, coming in there. It is very complicated until you know in each instance just where you are.

Pace: We found the same type of response, however, by using amyl nitrite which we have been doing more recently. Also Gellhorn* feels that the mecholyl response is a measure of sympathetic activity. So I certainly won't argue the point.

*Personal communication.

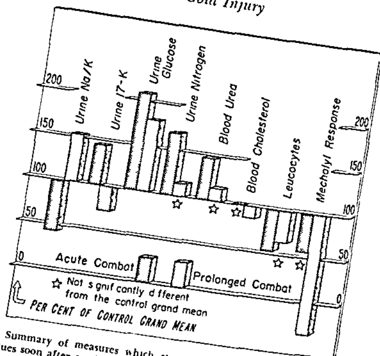


FIGURE 43 Summary of measures which statistically were significantly different from control values soon after combat in either of both acute and prolonged combat groups Values are expressed as a percentage of control grand mean for each measure Reprinted by permission from Pace N Schaffer F L, Elmadjian F, et al *Physiological studies on infantrymen in combat* *Univ California Pub Physiol* 10 1 (1956)

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*Gelhorn Ernst Dept of Physiology University of Minnesota Minneapolis Minn Personal Communication

For 4 days the men are out on their own With clothing rifle helmet rations (type C) pack and so on each man is carrying about 75 pounds Most of these Marines seemed to weigh only about 130 pounds so they are doing a lot of work I couldn t help being impressed by the fact that there is a relatively high incidence of frostbite among these men which admittedly shouldn t happen but it does

For example in the group of fifteen men that we studied in detail three got minor frostbite of the fingers from the usual thing—pulling off a glove not really believing what they had been told So these individuals got a very direct and realistic exposure to cold under a fairly typical military operating situation

The course has a real reputation among the trainees This is the climax of their training Many of the individuals that arrive for the course are exceedingly apprehensive about the whole thing So between the apprehension about the cold itself and the activities in which they engage I think it is a reasonable simulation of what military operations would be in a real life situation

Figure 44 shows the kind of results we obtained from these individuals The upper portion of the figure gives the data several days before exposure to the cold There are two sets of bars for each constituent As indicated on the figure the cross hatched bars are measurements obtained after the ntramuscular injection of a dose of 80 units of ACTH in gel So the action of the ACTH is spread out over a period of time

This is an expansion of Thorn s (3) ACTH test in effect to measure the responsiveness of the adrenal cortex to a massive dose of ACTH

These individuals reacted in the expected fashion to the ACTH There were some indications that the men were not in ideal physiological balance before being exposed to the cold For example sodium and chloride excretion rates were somewhat low whereas potassium output was relatively high Whether this represented some kind of aldosterone hyperactivity I don t know But by and large the other conditions were quite normal On injection of the ACTH the men responded in the expected fashion

Bass Did you follow the pH s?

Pace They ran around 5 5 or so There was nothing exciting about them

Keller These are group averages?

Pace Yes

Keller You did ACTH injections on each individual?

Pace Yes During the exposure to cold we got them at day 3

Keller It is, if there are no other variables

Pace At any rate, this is the kind of metabolic pattern we obtained in the combat study Since then we have made similar studies of various situations where individuals were exposed to cold

The first of these I shall discuss is a study we made early in 1957 on trainee personnel at the U S Marine Corps Cold Weather Training Base in the mountains of California, at a place called Pickle Meadow Marine trainees slated for assignment to cold weather areas are routinely sent from Camp Pendleton when they finish their combat training course in the wintertime to receive a 6 day indoctrination course in cold weather practices before going overseas *

The men are issued their cold weather clothing and receive lectures on its use before leaving Camp Pendleton for a 12 hour bus ride to Pickle Meadow Generally, when they get out of the bus there is anywhere from 1 to 3 feet of snow on the ground The station is at an altitude of about 6500 feet It was chosen deliberately as one of the coldest locations in the Sierra Nevada

Talbott What time of the year is it?

Pace It is in the winter they are in actual winter conditions

Talbott In what sort of climate do they undergo their Camp Pendleton training?

Pace That is in a very temperate location San Diego is a winter spa

Talbott So they are only acclimated to temperate climate?

Pace Yes They get out of the buses and, as I say, there is 1 to 3 feet of snow on the ground They form in pairs, pitch tents and put on the cold weather clothing Then their indoctrination begins

They are given one day at the base camp, sleeping in their own tents during which time they receive additional lectures On the second day they take off on tactical problems, and operate as units They are subjected to harassment at night by permanent members of the camp and have to defend the perimeter of their camp against these raids

Blair What is the range of ambient temperature?

Pace It varies but it can be as cold as −40°F , or as high as 40°F when the weather is unfavorable for this kind of training At the time we studied the group temperatures ran from about −10° to 0°F

*This study was carried out in collaboration with U S Naval Medical Research Unit No Two University of California Berkeley

the expected increase in 17 hydroxycorticoids as it did both before and after the exposure period I can't believe that the adrenal cortex in these individuals reached the limits of its responsivity

None of the other factors measured here indicates much variation from the normal pattern certainly not the kind of response that was seen in the individuals acutely exposed in the combat situation The general impression that one got was that these individuals subjectively at least were not in bad condition In fact by the end of the test in typical fashion the trainees are always very proud of the fact that they have now become veterans of the experience and give no indication of exhaustion Certainly I would never be able to interpret this as anything remotely resembling adrenal cortical exhaustion We have no explanation for the fact that we did not see an ACTH response in this group

Keller Do we have any decisive information relative to utilization of corticosteroids?

Pace Yes As a matter of fact we have some experiments on that point I think they are significant This is really a repetition of an experiment carried out by Sundstroem and Michaels (4) almost 20 years ago which seems to have been lost sight of by many people Unfortunately they were forced to use the old crude adrenal cortical extract and consequently the results were a little difficult to interpret

What they did was simply adrenalectomize rats and determine the minimum dose of adrenocortical extract required to keep them in good condition Then they took the adrenalectomized rats and exposed them to various altitudes in a low pressure chamber and again determined the minimum maintenance dose of adrenocortical extract They found a very nice direct correlation between altitude and the amount of adrenocortical extract required

Dr Timiras (5) of our laboratory working at the White Mountain Research Station has now confirmed and extended their findings using pure hormones I don't think there is any question that an increased demand for hormone under stress has been well established at least in this situation

Hart Is this after the animals are acclimatized?

Pace She has done both and has found several interesting things The animals exposed acutely to the altitude require more hormone than animals acclimatized prior to adrenalectomy On the other hand the acclimatized animals don't come completely back to normal but only about midway

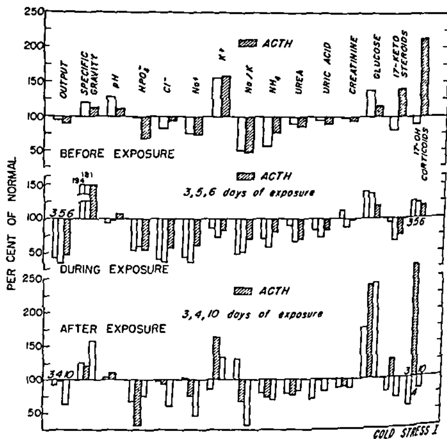


FIGURE 44 Mean urinary constituents of fifteen men before during and after participation in the 6 day U S Marine Corps Cold Weather Indoctrination Course at Pickle Meadow expressed as a percentage of the normal value for each constituent Cross hatched bars indicate response to 80 U.S.P units of ACTH in gel administered intramuscularly

which is really their second day of exposure to the stressful situation We caught them again at day 5 for a repeat, shown by the second bar Then on day 6 the last day, we gave them another ACTH test to measure the responsivity of the adrenal cortex The results are shown in the middle portion of Figure 44 The 17 ketosteroid excretion was reduced on day 5, in spite of the fact that the 17 hydroxy corticoid excretion was slightly but significantly elevated

Another interesting thing about this set of data is the fact that the injection of ACTH on day 6 of the exposure period did not evoke

Taylor The data were gathered under the most difficult circumstances imaginable

Pace Control samples were obtained from these men in Davisville before they left the United States Vaughan and Parker accompanied them down on the U S S Glacier and were able to carry out a number of physiological studies in addition to collecting the blood and urine samples They measured such things as flicker fusion frequency step-up test the amyl nitrite response ACTH response etc and they learned a lot of environmental physiology the direct way apart from the results of the studies

What is shown in Figure 45 is simply the metabolic pattern shown by tractor drivers taken after a typical 12 hour work stretch during which they had been out on the ice all day engaged in the off loading from the ships As Dr Taylor mentioned the men were out some 40 miles and it was an all day job for a tractor driver to go out get a load and bring it back into the base While the temperatures were not extremely low nevertheless this was a very cold job I don t think there is any question but that they were exposed to moderately severe cold

Taylor It was cold and a lot of the time it was overcast and blowing hard with a lot of snow in the air as well

Davis These men of course were engaging in a considerable amount of physical activity

Pace They were primarily sitting on the tractors This takes a certain amount of work

Taylor It would vary considerably from trip to trip

Pace I would guess they were on 4000 to 6000 calories per day

Keller Did they help load?

Taylor They didn t load but if the load fell off on the trail they had to help put it on

Davis These men were clothed?

Pace Yes

Hart Were you able to get body temperature or skin temperature? How do we know they were subjected to cold at all?

Horvath How do they compare with ordinary drivers in this country?

Pace We weren t concerned with this What we wanted to know was how much of an effect this situation had It was an actual situation where the men were operating in the cold in a typical fashion We simply asked the question In what physiological condition are these men? We did not try to solve the problem before we decided whether there was a problem

Hart Is this based on maintenance of body weight after adrenalectomy?

Pace She examined several factors liver glycogen, blood glucose plasma electrolytes and survival time, as well as body weight

Keller She examined rate of growth, too, didn't she?

Pace Not in this particular study As Figure 43 shows, the evidence we have indicates an increased excretion rate of hormone despite the apparent need for more hormone

Keller What is your concept of the reason for this response?

Pace One possible explanation is that there may have been a difference in the contribution by the androgens Another is that there are so many places the intermediary metabolism of the adrenosteroids could be affected, a reduced conversion rate to 17 ketosteroids could come almost anywhere

Figure 45 shows some of the results in the Antarctic This was a study made on some of the Seabees that Dr Taylor discussed earlier these men were engaged in constructing the U S Navy base at McMurdo Sound in early 1956 In fact, Dr Taylor was there The blood and urine samples were collected by two men from our laboratory, Dr Burton Vaughan and Dr Howard Parker Dr Taylor was most helpful to them in accomplishing the project

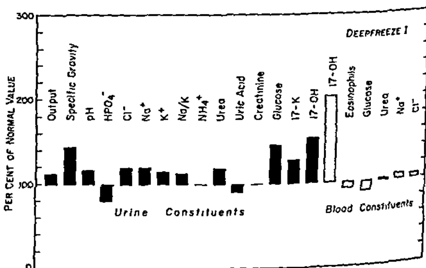


FIGURE 45 Mean blood and urinary constituents of 12 Operation Deepfreeze 1 personnel during off loading from U.S.S Glacier by tractor across the shelf ice at McMurdo Sound in Antarctica expressed as a percentage of the normal value for each constituent

Henschel But they could still be warm and experience frostbite

Pace These are men operating the way they would be in a practical situation We asked ourselves the question How much of their physiological reserve are they drawing on?

Davis This has usually proved disappointing

Burton Have you ever done a similar experiment on someone in your laboratory at 60°F naked?

Pace I am coming to something similar although I haven t done precisely that

Talbott I would like to ask Dr Siple whether or not he thinks these individuals were exposed to the cold

Siple Certainly by comparison to what they were accustomed to they were exposed to cold They were surrounded by cold machinery On the other hand some of the days during this period were up to 40°F With average clothing being worn sweating was a common occurrence especially while walking

Pace Not during this period This was in December right after their arrival

Siple That is what I mean I was present at the same period you refer to and the temperature was generally close to freezing but seldom if ever down to zero

Pace They were careful to take situations where the men were out on cold snow blowy days

Horvath These Cats were enclosed weren t they?

Pace No

Taylor The sides were closed but the roof hatch was left open after one vehicle went through the ice

Pace We simply asked ourselves how much of an indication of drawing on the physiological reserve they exhibited

Keller There are days in the Kentucky climate when a Cat operator will not work because of the cold

Burton Dr Siple told us that in his situation which was much more severe they could operate for only one hour and then go in the hut How long did these men operate the Cats?

Pace For 12 hours

Burton It shows the distinction very clearly doesn t it?

Horvath Is this the same type of operation Dr Taylor described? They went out for an hour and then stayed inside for another hour

Taylor Not the ones Dr Pace is talking about Sometimes they were traveling 12 hours or more

Pace This was in the off loading of these ships across the shelf ice

Buss Dr Pace I recall a rather large report that came out from

Davis Have you a control group in this country?

Pace Yes we got the same men before going down to Antarctica They were perfectly normal

Horvath Did you get them when they were operating the machines doing the same kind of work at Davisville? What happens?

Pace Not much happened to these men

Keller You feel the predominant stimulus was cold?

Taylor They were hungry most of the time

Horva [illegible] insult?

Pace [illegible] adrenal cortex a [illegible] n of 17 hydroxycorticoids was elevated by some 50 or 60 per cent above normal This also is reflected in the high blood level of 17 hydroxy corticoids in this instance Neither of these represents in our experience a major rise in these constituents although again this is highly significant statistically

The eosinophils showed essentially no response By and large the entire pattern is one showing what in our opinion is a relatively small response of the adrenal cortex in this situation

Taylor Is the normal value here the mean normal value or the normal for the individual as tested in Davisville?

Pace The results are the same no matter how we compare them because the control values at Davisville were very close to the normal population figures

Horvath What is the difference in interpretation between some thing highly significant statistically and something which you say is minor change?

Pace One is the matter of preciseness of the data The other is a matter of the degree of change You can have a small difference that is highly significant It has nothing to do with the degree of the change you are measuring The significance is simply a measure of the spread of the population about the mean value in the in stances and not just the difference between the units So we have these two cold situations and I must say I was quite disappointed by all this Here were two fairly rugged real life situations that did not seem to reveal any major response on the part of the adrenal cortex

Davis Were these people exposed to the cold do you think? That is was the microclimate under the clothing cold?

Pace The Marines were almost literally dumped off buses into zero weather They were frostbitten

Henschel But they could still be warm and experience frostbite

Pace These are men operating the way they would be in a practical situation We asked ourselves the question How much of their physiological reserve are they drawing on?

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Pace This was in the off loading of these ships across the shelf ice

Bass Dr Pace I recall a rather large report that came out from

the Medical Nutrition Laboratory in 1948, by Johnson and Kark (6) in which they flew men up to Shiloh They posed the same question to themselves, although they were studying restricted rations I don t remember the results in detail However, I do recall they appeared to demonstrate some aspects of what they called the alarm reaction comparable to the general adaptation syndrome They concluded that they had demonstrated the situation you were disappointed not to find

Pace We were only disappointed in the degree I think it is quite clear there was a response here

Keller That group had several variables In their conclusions they assumed that the change in external climate was the predominant variable

Bass One well fed group served as control

Siple It is fair to say that Parker and Vaughan took advantage of the only circumstance they had at the time to work with The men they studied were under as great a stress as anyone, although this is not to be interpreted as deep cold stress

Pace Deep cold is a short term kind of stress

Siple It can be extensive

Pace You are limited by definition

Horvath You just called this a stress The implication of this deep freeze type of stress is the thing we worry about more and more It may be under condition of operation entitled Deep Freeze but this may not necessarily be a deep freeze situation

Bass You say the men complained of being cold Were they shivering? Were any observations made on that?

Taylor My recollection is that they weren t cold enough to shiver

Siple I doubt that they shivered much at all

Bass Were their feet numb?

Keller Were they cold enough to flail their arms to keep warm?

Davis What do you think their metabolic rate was? Do you have anything on that?

Carlson Was the estimate a 6000 calorie day?

Pace It was 4000 to 6000, it wouldn't be any more than that From measurements that have been made of comparable activities I would guess it was that, roughly

Barquist This raises the problem of many field studies in the Arctic There are a number of variables in the measurements you observed and whether cold is even the most significant one I don t believe is established

I have been more cold this morning in this room since I wore

short sleeves than I was in 200 miles of ice cap travel in Greenland last year My skin didn t feel as uncomfortable as it does while I am sitting here

Pace I agree wholeheartedly In fact that is the point I would like to make When we are dealing with a practical situation what we want to know is what is happening to the total organism not fix our mind on one factor in the environment that we think *a priori* may or may not be important

Keller I would like to commend Dr Pace for this type of study The person who says there isn t any field work to be done these days is shortsighted This is the sort of work that has to be done before conclusions can be drawn relative to field activities

Pace I hope I don t leave the impression one does only field work to understand what is occurring There is absolutely no substitute for working these things out in the laboratory where the variables can be controlled But both sides of the picture are necessary in order to evaluate a real life environmental situation

Horvath On the one hand if one states specifically there is no little change here this can lead to the impression this particular situation is modified only slightly but not producing serious change

Fremont Smith This is the thought of anxiety This can t be cold because it can t be a real change

This would be too disturbing to investigators who have found changes in cold I think this is an extremely important point If you look at it the other way you can make all kinds of studies of extreme cold and bare face bare chest cold They may have nothing to do with the practical situation that the men are going to be in

So we must do both sides Then we must find out if these inter digitate

Pace One firm conclusion that can be drawn is that the Army Quartermaster has done a remarkable job of protecting the indi vidual against cold climates which I think is the Army Quarter master s job

These studies show that men operating in typical cold weather situations can be expected to perform as effectively as men operating in temperate situations The cold weather environment from an operational point of view has been largely whipped This has nothing to do with interest in the phenomenon of cold weather acclimation

Horvath This can only be stated if you give a very precise defi nition of the situations under which these experiments were per formed What is lacking at the present time is a precise statement of

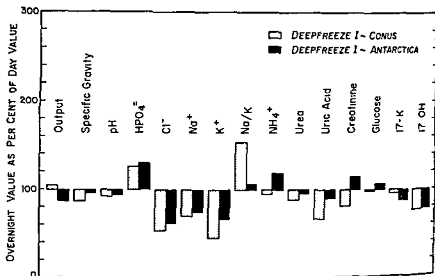


FIGURE 46 Mean urinary constituents of 12 Operation Deepfreeze I personnel during night period (7 00 p m to 7 00 a m) in Antarctica and during the same night period in the United States before departing for Antarctica both expressed as a percentage of the normal daytime value for each constituent

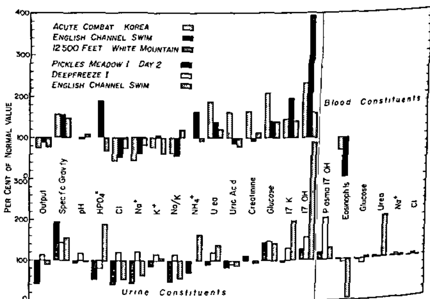


FIGURE 47 Summary showing mean blood and urinary constituents from groups of men exposed to a variety of environmental situations expressed as a percentage of the normal value for each constituent

the environmental conditions under which these tests were conducted

There is some contradiction between the feelings of some who were there and some statements you have made as to these precise conditions If we could only agree as to the precise situation the interpretation of the results that you have given us could be made much more logically

Fremont Smith Mild cold weather environment?

Horvath The kind of environment is not important The fact that a man was shaken on the machine might have been a factor Vibration is just as important

Pace But nothing happened [Laughter]

Keller There is another interpretation other than the one you gave about the Quartermaster and that is cold may not constitute a direct adrenocortical stimulus it may activate the adrenal cortices only indirectly such as when facilitation of gluconeogenesis is needed or when it is sufficiently severe to produce actual systemic trauma

Pace A question came up earlier in the Conference about diurnal variation in the continuous daylight of the Antarctic We did analyze urine samples that had been collected both during the theoretical night hours—the usual sleeping hours—and the daytime hours The results shown in Figure 16 reveal that the men exhibited the same kind of diurnal variation after they had been in the Antarctic for about 6 weeks that they showed in the United States before going there In other words grossly there was no demonstrable major change in the normal diurnal variation in excretory rate of the substances shown in Figure 46

Figure 47 summarizes much of the data that we have obtained Those I wish to draw particular attention to are repeated in the lower and up|
pattern
swim
anywhere from 12 to 20 hours Admittedly the situation is complicated by the fact that a large amount of physical exertion is involved In this case it is truly very large plus a possible cooling effect

The main point I wish to emphasize however is that this shows the extent to which an environmental situation can lead to a tremendous increase—something like a fourfold increase—in excretion of 17 hydroxycorticoids and other comparable changes In this case also we did see a complete disappearance of the eosinophils It was a quite remarkable thing to look at these bloods

In the bottom portion of Figure 17 are also shown the other two

situations I mentioned the Pickle Meadow Marines and the Deep-freeze I personnel The response was much smaller in these two instances

In the upper portion of Figure 47 are also repeated the combat data, for comparative purposes, together with some data we obtained on relatively acute exposure to altitude The duration of exposure to the environmental stressor in all cases was roughly comparable

Carlson *Would you mention again the bars that you feel are physiologically significant?*

Pace We can focus on the 17 hydroxycorticoids As I said earlier, it is not good to choose any one substance as a criterion, but this is a convenient one and certainly reflects one thing, namely, the activity of the adrenal cortex itself This is essentially the basis for the statement I made earlier, that I don't feel either of these other two real life, practical cold weather situations produced much of a response in the adrenal cortex

Carlson In the case of the English Channel Swim, we will say the adrenal cortex is driven but not utilized, whereas in the other cases *it might be highly activated but not utilized*

Pace I think that is answered by the White Mountain situation where we measured the adrenocortical hormone requirement in adrenalectomized rats and demonstrated an increased demand But in the intact animal, we also see, at the same time, increased blood and urinary excretion levels I think the two go hand in hand

Horvath You have here a situation in which the plasma 17 keto-steroids are very low in the Channel swimmers but very high in the urine However I would say that for the Deep Freeze operation they are higher in plasma than they are in the urine

Pace You are quite right This is precisely why we like the urine far more than the blood for estimating stressful situations When you take a blood sample, you are getting a representation of a particular moment, whereas with the urine, you are integrating over a period of several hours

Keller You are certainly correct in your interpretation in the instance of glucose The presence of glucose and the amount in the urine has a specific significance

Pace I think this principle applies to many or all of the constituents shown It is far more informative to examine excretion rates in the urine where one wants a summation of effect over a period of time

Fremont Smith You will have to have both a curve of the blood

er time and excretion rate of the urine over the same period of ne

Pace Ideally that is exactly right however in field situations it extremely difficult to collect serial blood samples It is fortunate at it isn t the other way around because it is far easier to collect ine specimens than blood samples

Fremont Smith It seems to me the organism has certain priorities homeokinesis It is more important for the organism to keep rtain structures constant than others This also may vary possibly nder different circumstances The organism also has a series of omeokinetic mechanisms that it can throw into play one after an ther under stress That is why I use the word product rather than summation

I think that the best example is L J Henderson s (7) volume on lood and the seven mechanisms operating to maintain the pH of e blood constant under stress of acidity That volume brings this ut very nicely

We really must content ourselves with the recognition of the ery much greater complexity of the organism than we have any dications of from this kind of study

This is an enormously important beginning step toward a very omplicated procedure which we will not fully understand for quite ome time We must see that when one mechanism fails another one thrown in Unless it is possible to see which one is operating at a iven point the given figure may be quite meaningless because it ay be the response to the second or the third mechanism which is hrown in and not to the first

Pace Yes By way of amplification I think the adrenal gland is a ery nice example of this because it seems fairly clear that when an rganism is subjected sharply to a stressor the response is the classic annon alarm reaction involving the sympathetic system and drenal medulla This is not sustained however It appears that the drenal cortex seems to represent a second mechanism that comes nto play timewise It seems to require of the order of a few minutes o half an hour of exposure to a stressor for the adrenal cortical re ponse to be detectable

This was brought out I think very clearly by the work of George Thorn and his coworkers (8) several years ago when they examined the effect of exercise on adrenocortical activity They studied the Harvard crew during a Harvard Yale boat race and came to the conclusion that exercise *per se* did not evoke a maximal adreno-cortical response

Bass By the index which he used which was based mainly on eosinophil counts

Pace Yes But more recently a very nice study by Redfearn *et al* (9) in Britain utilizing the treadmill for the exercise showed clearly that there is a direct correlation between length of exercise and degree of adrenal cortical response From their data it appears that 20 minutes which is about the duration of a 4 mile boat race is not enough time for the exercise effect to be maximal

There is still another response beyond the short response of the adrenal medulla and the more prolonged response of the adrenal cortex

The thyroid is an example of a system that requires days to weeks to be brought fully into play So I think it is essential to keep in mind the fourth dimension of time as well as intensity in characterizing the response of the total organism to environmental stressors

Talbott Were you able to carry out any thyroid studies as a function of time?

Pace No not yet We are hoping to add protein bound iodine to the constituents analyzed as well to try to get at thyroid function We are also in the process of adding epinephrine norepinephrine and 5 hydroxyindoleacetic acid determinations so that we may make some kind of estimate of the short term responses

Bass While a temporary member of our group Dr Sydney Ingbar in two separate studies assessed thyroid function in men who lived at 60°F nude for 2 weeks We first assessed thyroid uptake and rate of formation of thyroid hormone In the second study we assessed peripheral utilization of thyroxin In the first study we found an increase in rate of formation of thyroid hormone In the second there occurred a significantly elevated peripheral utilization For the peripheral utilization we used tracer doses of I labeled thyroxin

Fremont Smith How soon did it come into play?

Bass It was manifested in 4 days You mentioned protein bound iodine Here you may be going up a blind alley We found no change in protein bound iodine The men were cold In connection with that of course I am reporting on some aspect of these studies later but I mention it at this time because it is apropos We also attempted to assess adrenal cortical activity Here we thought we had the pure preparation of cold stress We found no change in eosinophils or 17 ketosteroids

If I might add one more comment I am losing confidence in eosinophils as used in healthy men despite what you report the

British workers found Some years ago we really heat stressed a group of men and exercised them to the point where they almost collapsed We followed the eosinophils immediately at the end of exercise in the heat and at 1 2 and 3 hours after we found no change in eosinophils

Behnke You were measuring relative number not total number

Bass We were counting them directly

Behnke You might have had a change in fluid volume

Bass We corrected for hemoconcentration

Pace Hemoconcentration would lead to increase

Burch Don t you think you must do tracer studies to record rates of metabolic turnover?

Pace This is turnover in the physiological rather than the metabolic sense

Burch It is really the excess that accumulates in the blood and urine

Pace The premise is this If the concentration of any substance in the blood increases a fraction of it is going to leak over into the urine

Burch The amount must vary however

Pace I think it is true for many of the organic metabolites There is an amazing number of substances in the urine It is a quite faithful replica of the plasma

Irving When I look at some of the figures for the specific gravity volume and electrolytes in the urine it seems the stress situation modified them in the same direction as sleep In bringing up that analogy between stress and sleep I am not joking Is there some comparable machinery or process at work there?

Pace No I think the explanation is much simpler than that I think it is just that when these people are in stressful situations they don t take time to drink water Generally water is not readily available I think it is just a simple concomitant of any rugged environmental situation

Irving It is not an indication of genuine stress

Pace I don t think so In the White Mountain experiments for example where we saw a very nice response of the adrenal cortex the subjects were in comfortable quarters with adequate food and drink and there was no indication of reduced urine output

Burton An important point that has come out from the criticism is the caution we have to exercise in using any one index to judge the physiological activity in a situation where there is a formation of substance or input of substance a blood level and an excretion

I was searching my mind for an analogy which will make this very

clear I think a good one would be to take the case of heat stress Suppose we decided, in order to get an idea of the need for water exchange, to measure the water in the urine If we did that we would find a great decrease in the output of water in the urine But unless we knew that there had been a great increase in the output and utilization of water in sweat, we would reach quite the wrong conclusion I don t believe you can really conclude anything

It is a most valuable, interesting observation to find this big change in the ketosteroids in the urine in these situations, but surely one cannot, on this data alone and without knowing the real turn over, conclude this means an increased adrenal cortical activity? It might be a decrease in utilization

So it is very difficult, in these steady states and disturbances of the steady state, to know what any one observation really means isn t it?

Pace Yes This is precisely my point in saying that it is only asking for trouble to use just the 17 hydroxycorticoids or just the eosinophils or just two of them

Burton Or just the urinary output of anything

Pace In that sense, we have set practical limits We have to start somewhere You see, by looking at these other metabolic constituents—and this I didn't get into—what we are really looking at are such things as protein metabolism, carbohydrate metabolism and purine metabolism, by measuring such things as amino acids urea creatinine, uric acid, glucose, and so on, in the urine This is exactly why we have included these substances, to make what attempt we can at our present level of knowledge, to interpret the data from a manifold point of view, to try to get at the different turnover rates again using this in the physiological sense of turnover rather than the classic biochemical turnover which, if it could be done practically certainly, also should be done

Fremont-Smith If you specify your assumptions on going through this process I think you are on perfectly safe ground because then others cannot criticize you Also, they can examine each assumption to see whether it is validated by other assumptions later on

The only danger is where one doesn t specify what assumptions one has made There is a whole series here and probably some unknown assumptions have to be made here These could be listed so far as possible Then you are on very safe ground and each one can be tested later

Keller I don t think we should close this discussion of the possibility that cold *per se* activates the adrenal cortices without mention

ing the recent and significant work of Egdahl and Hume (10) who cannulated and collected blood from the adrenal vein in the unanesthetized dog during cold exposure They found that adrenal cortical or medullary elaborations did not appear in adrenal vein blood during cold exposure

Later Richards (11) from the same laboratory found that sufficiently severe cold did cause a transient elaboration of adrenal corticoids into the adrenal blood but not continuously as surgical trauma does This correlates perfectly with our finding that exposing the adrenalectomized dog to a −20 C air temperature for several days did not precipitate an adrenal insufficiency crisis providing blood sugar was not allowed to fall sufficiently to cause a hypoglycemia crisis These animals are of course hypersusceptible to hypoglycemia in the cold due I suppose to decreased rate of glyconeogenesis

Pace I thought that Egdahl and Richards at Bethesda (11) found the opposite But perhaps this was with anesthetized animals?

Keller Richards work followed Egdahl s work using a greater cooling load but the adrenal cortical response was transient it was not continuous as occurs following surgical trauma

Lyman How cold were the Hume dogs?

Keller Hume subjected his dogs to as low as −40 C but the core temperatures of his dogs did not change from the homothermic level

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The colder environment with air temperature at −10 C but with a heat transfer coefficient of only one arbitrary unit could cause let's say minor frostbite In this environment the cooling power $\frac{Q}{At}$ would be 20 units

In the other environment with the air temperature +10 C the cooling power is twice as great because the *H* is twice as great and yet here we need not have any cold injury This suggests that we cannot necessarily obtain a good correlation between a biological variable (*e g* incidence of frostbite) and wind chill or cooling power

To understand a little better what we mean by cooling power I thought a comparison with electrical units might be helpful All of the heat variables have their electrical analogues and I shall point out only a few here

Variable	Electricity	Heat
Current	$I = \frac{Q}{t}$	$\frac{Q}{t}$
	$= KA(E_1 - E_2)$	$= HA(T_s - T_a)$
Current Density	$\frac{I}{A} = \frac{Q}{At}$	
	$= K(E_1 - E_2)$	$\frac{Q}{At}$
		$= H(T_s - T_a)$
Power	$W = I(E_1 - E_2)$	
	$= \frac{Q}{t}(E_1 - E_2)$	$\frac{Q}{t}(T_s - T_a)$
	$= KA(E_1 - E_2)^2$	$= HA(T_s - T_a)^2$

It is obvious that what we have been calling atmospheric cooling power $\frac{Q}{At} = H(T_s - T_a)$ is analogous to electrical current density $\frac{I}{A} = K(E_1 - E_2)$ and not to electrical power Hence atmospheric cooling power is only a graphic expression and not power in the sense of rate of doing work The expression for the thermal analogue of power contains the square of the temperature difference As far as I know this expression has never been used in either thermal physiology or in thermal physics

After the relation of H to wind has been experimentally estab lished a scale of cooling power for different combinations of wind and air temperature can be drawn up on the basis of calculations of $\frac{Q}{At} = H(T_s - T_a)$ This can be done however only if either T_s or $\frac{Q}{At}$ is held arbitrarily constant Dr Siple chose to keep the bare skin surface temperature constant at a comfortable 33 C Dr Burton on the other hand chose to keep $\frac{Q}{At}$ constant at 38 kg cal /sq m /hr or some multiple thereof Both alternatives are equally valid in theory and a choice can be only arbitrary based on con venience objectives conditions etc Our further inquiry must there fore turn from theory to an examination of the data on the basis of which Dr Siple and Dr Burton erected their respective scales of cooling power

Dr Siple made his measurements in the Antarctic during the winter of 1940 (1) As shown in Figure 48 he measured the time required for a volume of water inside a plastic cylinder exposed to the outdoors to freeze I present this figure because many people have thought the measurements were made on men instead of on a physical object Also the object has been misquoted as being a tin can instead of a plastic cylinder

Air temperature was measured by means of the electrical resistance thermometer seen on the right in Figure 48 and wind velocity was measured by means of an anemometer stationed somewhere nearby It should be noted that at least for certain directions of the wind the pole and crossarms probably induced turbulence in the flow of air around the cylinder The wires from the resistance thermometers and from the anemometer went down to the room below where the observers operated the electrical instruments

There was no diagram of the cylinder in Dr Siple s report but we inferred from his description that it looked something like the outline in Figure 49 Inside the cylinder was a brass tube which was the housing for the electrical resistance thermometer The tube was held in the center of the cylinder by means of a large cork (shown as an oval in Figure 49) and the water level was below the top of the cylinder to allow space for expansion during freezing

The variables which had to be measured for the calculation of the heat transfer coefficient are indicated by the solution of the heat transfer equation for H

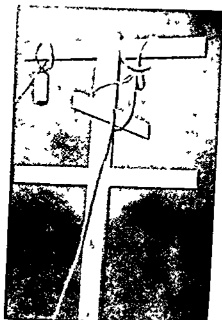


FIGURE 48 The Siple plastic cylinder suspended on the left from the crossarm of a pole above the Science Building at Little America III 1940 On the right is the resistance thermometer for measuring air temperature The cables led to instruments in the laboratory below (Photo by Charles Shirley U S N) Reprinted by permission from Siple P A and Passel C F Measurements of dry atmospheric cooling in subfreezing temperatures *Proc Am Philos Soc* 89 *177 (1945)*

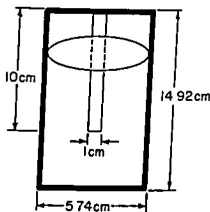


FIGURE 49 The Siple cylinder (diameter exaggerated for clarity) The resistance thermometer was in the centrally located brass tube The oval represents the large cork sphere which held the brass tube in the central position An air gap between the top of the water and the top of the cylinder permitted expansion of the water during freezing

$$H = \frac{Q}{At(T_s - T_a)}.$$

Apparently the air temperature T_a was measured with sufficient accuracy but Figure 49 shows that the surface temperature T_s was not measured since the thermometer was located on the central axis of the cylinder. This central temperature T_c was substituted for T_s in the formula. However T_c is always greater than T_s even during freezing so that the temperature value used in the denominator was too great.

The area A was assumed to be 1 sq cm for each gram of water. Since there were 250 gm of water in the cylinder the total area assumed was 250 sq cm. In fact however the total area was about 325 sq cm. The difference of 75 sq cm could possibly be assigned to the upper side and top of the cylinder not in contact with the water and could be ignored if no heat transferred from this surface. In our experiments the top end of the cylinder above the water was always warmer than the air. From this we concluded that heat was transferred from all areas of the cylinder and therefore that the value of 250 sq cm used for A in the denominator of the formula was too small.

Siple assumed that the quantity of heat Q transferred was only the latent heat evolved during the freezing of the water. We shall find that this was an inadequate estimate of Q because stored heat from ice was also released in these freezing experiments.

Because Q was to be only the latent heat of freezing and was not to include stored heat released before and after freezing the value used for time t was the time it took for the water to freeze. This was determined in the manner illustrated by the three sample freezing curves in Figure 50 obtained with different combinations of wind and air temperature. The water at the central axis was obviously freezing during the horizontal portions of the curves. The curves approach and recede gradually from the horizontal however so that the time of onset and termination of freezing is not precisely evident. In addition there is a little hump in the cooling curve preceding the approach to freezing. Dr Siple correctly accounted for this hump on the basis of change in the density of water at around 4 C. However both this hump and the gradual approach to freezing at the center occur only in unstirred water. In our experiments with stirring water cooled exponentially toward air temperature but upon reaching 0°C it abruptly stopped cooling and the time of onset of freezing could be noted with precision.

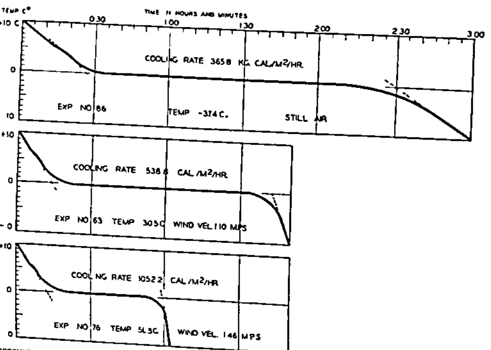


FIGURE 50 Sample freezing curves from Siple and Passel. They assumed that freezing started at the intersection of the dotted extrapolation with the zero degree horizontal on the left and was completed at the comparable intersection on the right. Reprinted in part by permission from Siple, P. A. and Passel, C. F.: Measurements of dry atmospheric cooling in subfreezing temperatures. *Proc Am Philos Soc 89: 177 (1945)*.

To ascertain the moment of onset of freezing the segment of the curve below the hump was extrapolated linearly to 0°C the intersection was taken to indicate the onset of freezing It will be demonstrated later that this was an insufficient correction that freezing at the surface started some time before this intersection of the extrapolated cooling curve with 0 C In like manner the termination of freezing was taken to be the intersection with 0 C of the extrapolation upward of the cooling curve for ice Again this correction was in error it postponed the end of freezing but thereby it helped to make up for the deficit of the correction for the initial onset of freezing Thus the ultimate estimate of time t for use in the formula was almost correct

To check all these fine points about the cooling and freezing of water we have conducted an extensive series of experiments in a small wind tunnel in the cold room of the Fort Knox laboratory We used cylinders approximating Dr Siple s in size and shape They were made of acrylate plastic copper or copper painted black Temperature was measured by means of thermocouples distributed as shown in Figure 51 In some experiments the water was stirred by means of a simple jiggling device

Figure 52 shows simple temperature curves obtained with a copper cylinder The air temperature was about −10 C and the wind

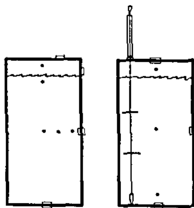


Figure 51 Cylinders used in the Fort Knox experiments The cylinder on the left features a lateral [illegible] of thermocouples seen [illegible] The cylinder on the right features an axial [illegible] of thermocouples attached to a string [illegible] which [illegible] horizontal [illegible] a thread attached to a rubber band at the bottom and jiggled by a motor pulley [illegible] of the wind tunnel) The rectangles on the [illegible] surfaces of the walls indicate the thermocouples cemented in place

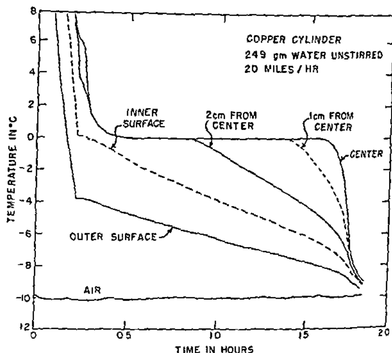


FIGURE 52 Example of temperatures in relation to time on a radial element in unstirred water Thermocouple distribution as in the left diagram of Figure 51 Copper surface

was about 20 miles per hour The outer surface cooled rapidly until it reached about −4°C, when it suddenly stopped cooling shortly thereafter it started to cool again but at a very slow rate The sudden halt in cooling at −4°C could have been effected only by a sudden large increase in heat flow to the surface at this moment The only source for this increment was the latent heat of freezing of water Hence the moment at which the surface cooling suddenly stopped was the moment of onset of freezing This moment was considerably earlier than the time at which the fluid in the center started to freeze

Freezing proceeded from the surface inward After crystallization was completed at any point, the temperature of the point then proceeded to fall slowly The onset of ice cooling migrated inward from the surface toward the center The temperature fall in the ice, of course, indicated the evolution of stored heat from the ice Hence while the center was freezing the total heat transfer included not only the latent heat of freezing but also the stored heat of ice

It is evident that the difference between surface temperature and

air temperature was much smaller than the difference between center temperature and air temperature It was the smaller $(T_s - T_a)$ and not the larger $(T_c - T_a)$ which drove the heat from the cylinder Also $(T_s - T_a)$ was continuously diminishing it was not a constant difference during the time of freezing as demanded by the formula This diminution prolonged the time necessary for the transfer of the total heat hence the formula requires modification to take this into account

Siple Are you certain that at that temperature range you weren t ge

the

su In this example there was no supercooling We never knew when to expect supercooling

Siple Where you show temperatures coming up they were flat for a way and then dropped off Is that correct?

Molnar Yes I did not want to clutter the graph with too many symbols or types of curves The temperature 2 cm from the center followed the curve for the center except that it humped lower at about 4 C thereafter it was superimposed on the curve for the center until its freezing was completed

Figure 53 shows examples of the course of cooling and the onset of freezing at the center and at the surface with both stirred and unstirred water When stirred the center cooled through the 4 C zone without humping and it started to freeze abruptly simultaneously with the surface without a gradual approach to 0 C In this example there is a hint of supercooling When the water was not stirred the temperature fall at the center showed a distinct hump and also a gradual approach to 0 C The extrapolation intersected 0 C at a point about 4 minutes after the onset of freezing at the surface

Figure 54 is an example of the onset of cooling at the center of a solid The temperature fall at the center of a solid always starts gradually In this example the water in the cylinder was immobilized by means of agar and the temperature at the beginning was purposely set way above the freezing point of water just to emphasize that freezing and ice formation are not *per se* responsible for the gradual rounding of the temperature curve at the end of freezing Since in this example cooling obviously started when the temperature first dropped below the horizontal line drawn through the initial temperature the intersection with the horizontal of the upward extra

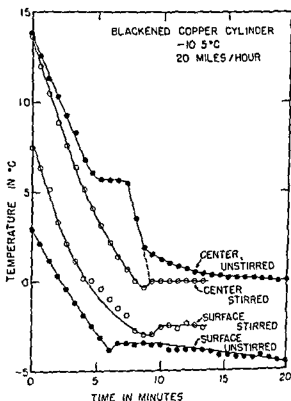


FIGURE 55 Example demonstrating the onset of freezing at the surface and at the central axis when the water was stirred and when it was unstirred

polation from the steep segment of the curve cannot be taken as the moment of onset of cooling at the center of a solid, which in the case of the freezing experiments was ice

Horvath You sıy this is representative of a solid This is still 5 per cent solution isn t it?

Molnar It was a solid in the sense that in the agar gel there were no thermal convection currents to stir up the water Hence, as in a solid, heat could move from center to surface only by conduction

We have already seen that the diminution of $(T_s - T_a)$ during

t nm-

m

5

They show the course of $(T_s - T_a)$ during freezing for [illegible] al cylinder and for a plastic cylinder Although about the same quantity of heat transferred from both cylinders, it took about 2½ times

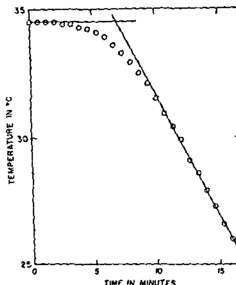


FIGURE 51. Demonstration of the convexity in the temperature curve at the onset of cooling at the center of a solid cylinder (5 per cent agar in plastic cylinder). Air −10.7°C, 20 miles/hr.

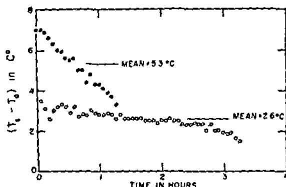


FIGURE 52. Surface to air temperature difference during freezing of unstirred water. ● = blackened copper cylinder, [illegible] miles/hr; o = plastic cylinder, 17 to 18 miles/hr.

as long for the transfer to take place from the plastic as from the metal cylinder. This was due principally to the fact that, because of the resistance of plastic to the flow of heat, $(T_s - T_a)$ was on the average only half as great for the plastic as for the metal cylinder. This is perfectly all right if, in the calculation of the heat transfer

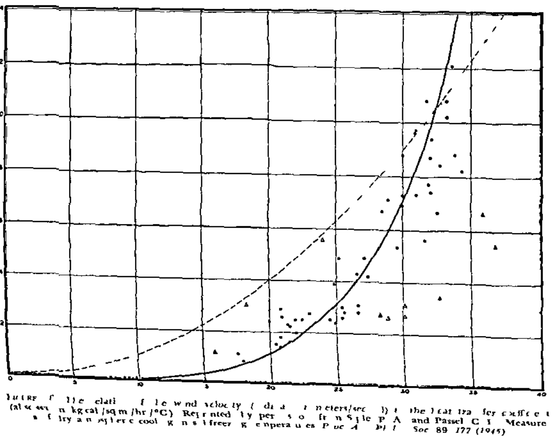


Figure [illegible]. The relation of the wind velocity (ordinate, in meters/sec) to the heat transfer coefficient (abscissa, in kg cal/sq m/hr/°C). Reprinted by permission from Siple, P. A., and Passel, C. F.: Measurements of dry atmospheric cooling in subfreezing temperatures. Proc. Am. Phil. Soc. 89: 177 (1945).

coefficient the value for the surface temperature is used in the formula. However, the time value obtained with a plastic cylinder is extraordinarily large if the central temperature at 0 C is used in the formula, as it should not be. So the plastic added a great deal of error by prolonging the whole procedure.

To summarize the critique of the data, we have found the following: the values for Q in the numerator of the formula were too small because they did not include the stored heat evolved from ice; the denominator was too large because, although the area A was undervalued, the use of the central temperature instead of the surface temperature caused the temperature difference to be too great; this error was compounded by the large value for the time for freezing resulting from the continuous diminution in $(T_s - T_a)$ and from the resistance of plastic to the flow of heat. With the numerator too small and the denominator too large, the resulting heat transfer coefficients, or the wind chill factors as Dr. Siple called them, must be considered to be too small.

The next matter to consider is the correlation between the heat transfer coefficient and the wind velocity. Figure 56 is from Dr. Siple's report. The wind velocity is on the ordinate and the heat transfer coefficient is on the abscissa. The dots represent acceptable data; the triangles represent data eliminated because of wide divergence or unreliability. The clustering of points between 2.5 and 3.5 meters per second was thought to be caused by anemometer difficulties or by turbulence and local convection currents. The dashed curve indicates a correlation with the square root of the velocity, $\sqrt{WV} \times 100$. It is obviously quite removed from the data. So Dr. Siple devised another formula

$$H = \sqrt{WV} \times 100 + 10.45 - WV$$

which resulted in the continuous curve; this went through the points quite well.

Figure 57 shows the same data replotted but with the wind velocity, since it is the independent variable, on the abscissa. The open circles are points not used in the fitting of the curve. The highest velocity of the wind was 27 miles per hour.

Figure 58 shows, as Dr. Siple himself has previously mentioned, that extrapolation of the formula beyond the data will cause the curve to turn back on itself. Thus a sufficiently high wind velocity would change the sign of the heat transfer coefficient. The data do not justify a formula which will do that sort of thing.

Burton: Is it not true that when the wind velocity is above 80 or

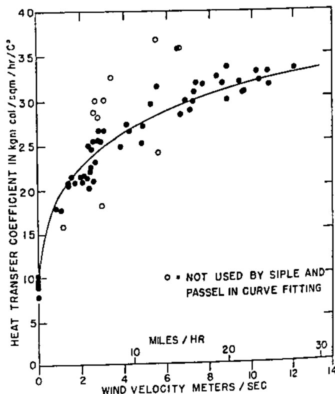


FIGURE 57 The Siple Passel data of Figure 56 replotted with the independent va on the abscissa The curve would have followed a steeper course if all of the had been used in the fitting

90 miles an hour there is this reversal? It may be an accident of empirical formula fitted over the range depicted, but this is a fact, isn t it?

Molnar I have always heard that if the wind is fast enough, will start picking up heat I haven't been able to find out anyt about this Do you have any information about it?

Burton No, simply that people in the Air Force Aerodyn laboratories told me this

Molnar Also, it is true that meteors burn up because they n through the atmosphere too fast I thought perhaps this forr would be acceptable for that reason On the other hand, if we wa be critical, there is nothing in the experimental data which just the formula Also, there is no theoretical reason for this form Finally, I couldn't find any statistical justification for it that is, p that this formula gave us the best type of curve through the dat

Siple I had one theoretical justification for the shape of the cu

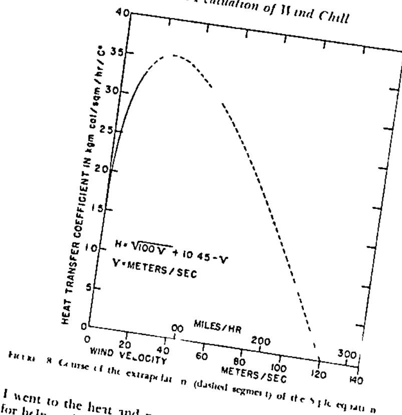


FIGURE 8 Course of the extrapolation (dashed segment) of the Siple equation

I went to the heat and power division at the Bureau of Standards for help and this was their interpretation of the curve and formula which best fit my experimental data

Molnar I wondered because I have looked at that formula again and again and I haven't been able to figure out how you arrived at it

Siple It was simply a formula that would come as close as possible empirically to fitting

Molnar These errors that I have pointed out demand some measure of their magnitude They may sum up to be insignificant in total effect So we thought we would try to see what the physicists could provide and also what we could find out for ourselves There is in the physical literature—and I know Dr Siple has been exposed to this (2 3)—a considerable amount of information that we in thermal physiology unfortunately generally disregard I present here one of several formulas this is the one most often used and it is the simplest one It equates the Nusselt number to the Reynolds number

$$N_{Nu} = B(N_{Re})^n$$

$$\frac{HD}{k} = B\left(\frac{DV}{\mu}\rho\right)^n$$

$$H = B\,\frac{k}{D}\left(\frac{DV}{\mu}\rho\right)^n$$

The Nusselt number is a dimensionless fraction containing in the numerator the heat transfer coefficient H and the cylinder diameter D and in the denominator the air film conductivity k The Reynolds number is also a dimensionless fraction in the numerator is the cylinder diameter D the wind velocity V and the air density ρ and in the denominator is the air viscosity μ B and n are empirical constants

By solving the formula for H we have a convenient means for calculating heat transfer coefficients for a series of wind velocities We have used the diameter of Dr Siple s cylinder for D The constants k ρ and μ are available in standard tables The constants B and n have been determined by careful experimentation by many workers The values published in McAdams book (4) and considered to be among the best are those of Hilpert (5) We have used Hilpert s constants

The results of our calculations are compared with Dr Siple s curve in Figure 59 For wind velocities above about 10 miles per hour the calculated curve rises steeply by comparison the Siple curve appears to be almost horizontal For wind at 50 miles per hour the calculated coefficient is almost 2½ times the Siple coefficient

The trouble with the calculated curve however is that the constants B and n used in the calculation were obtained from experiments in which the cylinders were equivalent to infinite cylinders The Siple cylinder however was definitely a finite cylinder It would therefore be desirable in order to evaluate the error in the Siple experiments to have heat transfer coefficients obtained directly from the Siple type of cylinder under his experimental conditions Hilpert and others obtained their constants under steady state conditions whereas the Siple experiments were not obtained under such conditions Hence it was necessary to derive a formulation applicable to unsteady state data

In the steady state the differential equation for heat transfer is

$$\frac{dQ}{dt} = AH(T_s - T_a)$$

All terms in the right member are constant In the unsteady state however $(T_s - T_a)$ is not constant because T_s is always diminish

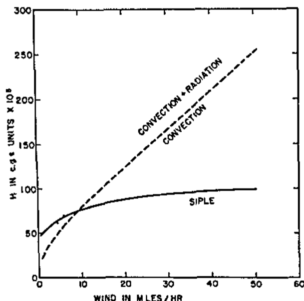


FIGURE 59. Relation of the heat transfer coefficient to wind velocity. The coefficients for convection were calculated by means of Hilpert's empirical constants (5) for a cylinder of the same diameter as Siple's cylinder.

ing. Under certain circumstances the value of $(T_w - T_a)$ at any time t is equal to the initial value of $(T_w - T_a)$ multiplied by a term which diminishes exponentially with time, or

$$(T_w - T_a)_t = (T_w - T_a)_{t=0}\, e^{-kt}$$

Here k is the slope of the semilog plot of $(T_w - T_a)$ versus time. The differential equation for heat transfer in the unsteady state is then

$$\frac{dQ}{dt} = AH(T_w - T_a)_{t=0}\, e^{-kt}.$$

Solving the integral of this equation for H, we have

$$H = \frac{Q}{A(T_w - T_a)_{t=0} \left[\dfrac{1 - e^{-kt}}{k}\right]}$$

This equation cannot be used for the interval during which the water was freezing because during this period $(T_w - T_a)$ diminished apparently linearly (Figure 52) and not exponentially. Preceding

$$N_{Nu} = B(N_{Re})^n$$

$$\frac{HD}{k} = B\left(\frac{DV}{\mu}\rho\right)^n$$

$$H = B\frac{k}{D}\left(\frac{DV}{\mu}\rho\right)^n$$

The Nusselt number is a dimensionless fraction containing in the numerator the heat transfer coefficient H and the cylinder diameter D and in the denominator the air film conductivity k The Reynolds number is also a dimensionless fraction in the numerator is the cylinder diameter D the wind velocity V, and the air density ρ and in the denominator is the air viscosity μ B and n are empirical constants

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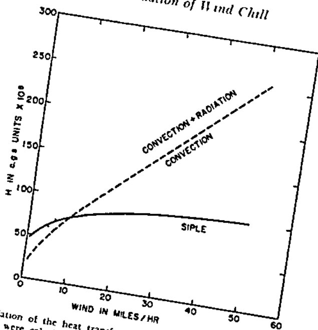


FIGURE 59 Relation of the heat transfer coefficient to wind velocity The coefficients for convection were calculated by means of Hilpert's empirical constants (5) for a cylinder of the same diameter as Siple's cylinder

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$$(T_s - T_a)_t = (T_s - T_a)_{t=0}e^{-kt}$$

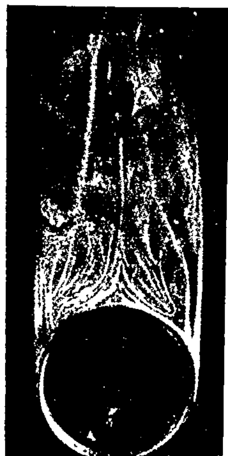
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$$\frac{dQ}{dt} = AH(T_s - T_a)_{t=0}e^{-kt}$$

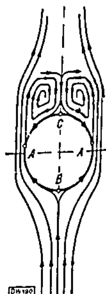
Solving the integral of this equation for H we have

$$H = \frac{Q}{A(T_s - T_a)_{t=0}\left[\frac{1 - e^{-kt}}{k}\right]}$$

This equation cannot be used for the interval during which the water was freezing because during this period $(T_s - T_a)$ diminished apparently linearly (Figure 52) and not exponentially Preceding



(*a*)



(*b*)

FIGURE 62 Turbulence on the lee side of a cylinder in laminar flow of air (in photograph striking the cylinder from below) The drawing on the right is a diagram of the air paths in the photograph Reprinted by permission from Lohrisch W Bestimmung von Warmeubergangszahlen durch Diffusionsversuche *Verein Deutscher Ingenieure aus Forschung Geb Ingenieur* 322 46 (1929)

cylinder The air flowed in laminar planes from below around the cylinder and then swirled into eddies on the leeward side Thus even with laminar flow there is turbulence but decidedly on only one side of the cylinder It is possible that in our arrangement we had turbulence all around the cylinder and therefore obtained heat transfer coefficients twice as great as those obtained by physicists with laminar flow

In our wind tunnel (Figure 63) the cylinder was mounted just

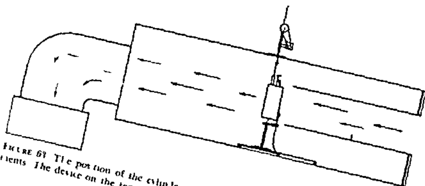


FIGURE 69 The position of the cylinder in the wind tunnel in the Fort Knox experiments. The device on the top jiggled the stirrer

beyond the end of an intake duct. We had wires and other equipment dangling around obviously many things were in the way even as mentioned earlier. Dr Siple had a post and crossarms near his cylinder. Obstructions always induce turbulence. We also had upper and bottom surfaces which added to the total rate of heat transfer. We were advised that probably the end of the intake duct caused most of the turbulence that we should try measurements with the cylinder placed inside of the duct. We have just recently completed these recommended experiments but the results did not differ significantly from the earlier values. This seems to indicate that the air flow was already turbulent in the intake duct.

From all this we conclude that (a) the magnitude of the values for the heat transfer coefficient as determined by Dr Siple is far too low (b) that the values we can calculate by using the dimensionless formula are acceptable for infinite cylinders in a laminar flow of air (c) that those values we obtained in our experiments are perhaps representative for the other extreme—a finite cylinder in highly turbulent air flow and (d) that perhaps under normal conditions of life we oscillate between our points and those of the Hilpert curve. Therefore it is difficult to say just what should be the heat transfer coefficient in any situation where men might be involved. This is apart from the problem of the size of the cylinder.

The Hilpert curve was recalculated on the basis of 9 inches for the diameter instead of the diameter of the Siple cylinder. The curve is shown in Figure 61. It is evident that the increase in the diameter brought down the values for the heat transfer coefficient very con

siderably Therefore, since the several segments of the body differ in diameter, the heat transfer coefficients for them will also differ that is, the body has several and not merely one heat transfer coefficient

Siple What was the diameter of your cylinder?

Molnar The same as yours, it was 2½ inches

Davis Could you have used an aerodynamic shape for the cylinder to cut down the turbulence?

Molnar The physicists used circular cylinders

Burton Did you compare your data on the cylinder with the data I obtained on the "artificial man," which was the size and shape of a man, because those data would apply to man This effect of the size of a cylinder has been nicely worked out here at Fort Knox You will remember that this is why it is impossible to make a glove that fits on fingers with any respectable insulation Haven't you compared these values yet with the standard values which I thought I had established for man?

Molnar I will conclude my remarks with such a comparison As a matter of fact, we are starting out on your ideas next

Dr Burton is unhappy because all of our considerations about wind chill thus far pertain to the nude surface and obviously we *are clothed Therefore, our estimate of wind chill should be for the* clothed man Perhaps it should be noted here that our considerations about the heat transfer coefficient apply to both nude and clothed surfaces It is the value assumed for the surface temperature that makes the difference with respect to wind chill For the same heat transfer coefficient, the wind chill for nude skin at 33°C would be considerably higher than for clothed skin at 33°C but with the clothing surface at a much lower temperature Hence, Dr Siple's scale of wind chill obviously cannot indicate the atmospheric cooling power for a clothed man Dr Burton has attempted to supply a substitute scale which would be informative for a clothed man

The derivation of his ideas is shown in Figure 64 which is reproduced from his book (7) Briefly, H, as used here, is the rate of nonevaporative heat transfer and not the heat transfer coefficient *Instead of the heat transfer coefficient, Dr Burton prefers* to use its reciprocal which he calls insulation There are two insulations here *I_{Cl} is the clothing insulation and I_A is the air insulation* W is the decrement in I_A from that of still air produced by wind

However Dr Burton, I couldn't find the origin of that 0 11 I calculated forward and backward to determine it, but with no success I think that it is an error Actually, what is needed is 0 18 in

For a thermal steady state using clo units for insulation and Met units for heat production or loss

$$M - E = 0.11 \times \frac{T_S - T_O}{I_d + I_A} \text{ for °C}$$

$$= 0.061 \times \frac{T_S - T_O}{I_A + I_d} \text{ for °F}$$

Since $I_A = I_{S.A} - W$

$$H = 0.11 \times \frac{T_S - T_O}{I_d + I_{S.A} - W}$$

$$H(I_d + I_{S.A} - W) = 0.11\,(T_S - T_O)$$

$$H(I_d + I_{S.A}) = 0.11\,(T_S - T_O) + HW$$

$$= 0.11\left[T_S - \left(\frac{T_O - HW}{0.11}\right)\right]$$

$$\text{or } H = 0.11\,\frac{T_S - \left(T_O - \frac{HW}{0.11}\right)}{I_d + I_{S.A}} \text{ for °C}$$

FIGURE 64 Burton's formulations for the calculation of the equivalent still air temperature Reprinted by permission from Burton A C and Edholm O G *Man in a Cold Environment* London Edward Arnold Ltd 1955

the numerator to convert W to thermal units *i e* from clo value to thermal units *

Carlson I remember being able to solve this Dr Burton converts to other units H is in Mets for example

Burton The end result means there is a possibility of calculating an equivalent still air temperature In other words if it is −20 C and there is a 25 mile an hour wind this tells you what to subtract from the −20 C to find the equivalent temperature for a man in which he would have the same degree of comfort and the same heat exchange as if the air were still That is that decrement thermal wind decrement HW × 0.11 that can be calculated for different winds

*EDITORS NOTE Dr ... would like to add the following afterthought to his remarks at the Conference

Dr Burton's coefficient of 0.11 ... formula (Figure 64) is correct when Met and clo units are used which he clearly indicated should be used ... terms of ... when ... as I have ... become accustomed to the Met and clo units

The important point, which was a new one to me, was that, according to algebra, the amount that must be taken off depends upon the level of metabolism, of heat exchange in the man. So, when the man has a level of 2 Mets of activity instead of 1 Met, twice as much must be taken off for the wind decrement as when he is resting and his activity of only 1 Met.

Fremont-Smith: Do you mean, literally, taking off clothes?

Burton: Subtracting the decrement. This dependence on the level of activity is logical. In an unheated building wind makes no difference to comfort as long as it doesn't get inside; however, in a heated building, wind does affect comfort inside. So it is logical that nomograms are necessary, depending on metabolic rate for this decrement, in calculating this effective still-air temperature. Separate curves are essential for different levels of activity, but this does not apply to this index, as it is to be used for predicting the degree of protective clothing needed. Here one must make separate predictions for the resting man, for the active man, and so on. But this device does theoretically get around the objection to the Siple wind chill, which is that it is obviously impossible to predict the heat loss and how it is affected by the wind, without reference to the amount of insulation the man has on. If he is wearing a lot of clothing, increasing the wind makes little difference to the total heat transfer. If he is naked, it makes a great deal of difference.

This effective "still-air" temperature is free from that objection. Imagine that it is −20°C and there is a 25 mile an hour wind, and this is equivalent to a 30°C still air temperature, it still must be taken into consideration how −30°C low would affect a man with different amounts of clothing. I would like to see this scale adopted and climatic maps drawn on the basis of still shade temperatures which seem to be free from theoretical objections.

Molnar: What Dr. Burton was talking about is shown in Figure 65. This is a replot of the figure in his book but with the independent variable, the wind speed, on the abscissa. The ordinate is the thermal wind decrement, that is, the temperature to be subtracted from the ambient temperature to get equivalent still-air temperature.

As he pointed out, with different levels of heat input, different amounts of wind decrement have to be subtracted from the ambient temperature. I feel that it would be confusing to read in a report that the observations were made in an equivalent still-air temperature, of let's say, −18°C. This could have been in an ambient air of −10°C for a sitting man. But, if that is true, then there could have

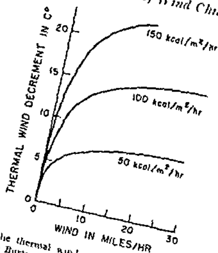


FIGURE 65 Relation of the thermal wind decrement to wind velocity. Reprinted in part by permission from Burton, A. C., and Edholm, O. G.: *Man in a Cold Environment*. London: Edward Arnold, Ltd., 1955.

been right next to him a working man for whom, however, the equivalent still air temperature would have been perhaps −56°C.

Burton: Often the truth is confusing.

Molnar: That is to me as confusing as always, remembering that Dr. Siple's scale refers to the nude skin at 33°C. Be that as it may, the theory is correct in both cases. The object here is to determine whether the factual foundation is all right in both cases in order to know whether to retain and use either one or the other scale.

I have already carefully gone over the factual foundation of Dr. Siple's scale. Dr. Burton's data are really data from the Pierce Laboratory at Yale University. This means that the data of the Pierce Laboratory should be examined with a critical eye.

The variable in Dr. Burton's formula, the foundation for his whole scheme, is the insulation of air, I_A. He uses 0.14°C/kg-cal/sq m/hr as the numerical value for this variable. It is necessary to determine how this value was obtained. In his book (7), in two different passages Dr. Burton cites two different references from the Pierce Laboratory (8, 9) as the sources of his information. I have looked at both of them. Neither one contains this 0.14 as so many numbers. Obviously then the value is implied by the data in those papers.

Burton: This is a particular case worked out. This is in a paper by Gagge, Burton and Bazett.

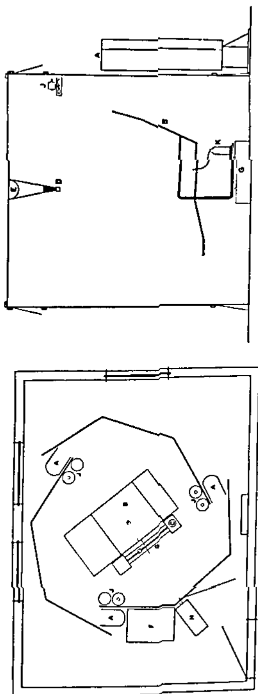


FIGURE 66 Horizontal (left) and vertical (right) sections of the experimental booth used by Winslow Herrington and Gagge

A = heaters
B = chair
J = 6 inch fans directed to the floor of the booth
G = platform scale
H = metabolism apparatus

Reprinted by permission from Winslow C E A Herrington L P and Gagge A P A new method of partitional calorimetry *Am J Physiol* 116 641 (1936)

Molnar In that paper (10) you indicated that the data were from the Pierce Laboratory It is one of these references (9)

Burton Also I would like to correct it although I used this formula to fit the right shape of curve my values for I_A with wind were verified on the artificial man in a cold chamber

Molnar As you recall the Pierce Laboratory workers used a special type of booth calorimeter as reproduced in Figure 66 from heir paper (11) The subject sat in the center In still air experi ients the air drifted in through three slots where the walls over pped In forced air flow experiments the flow was generated by ee pairs of fans at the top This flow was directed straight down a turbulent manner around the subject

orvath Directed to the core and the three sets were spaced at thing like 120 degrees apart and these were directed to the floor he side of the subject but not on the subject

lnar No not on the subject They measured the speed of air y means of a hot wire anemometer but they held it 6 inches ut 16 centimeters from the man at 15 points and averaged these measurements to have one value for air flow They repeated the same measurements without the man sitting in the chair The results compared as follows (*Table II*) (*8*)

TABLE II

Air Movement in cm sec

	Still Air		Forced Air Flow	
	A	B	C	D
Without subject	4 6	6 1	34	264
With subject	6 1	7 6	24	191

In still air the speed of air movement was greater with the subject in the chair than without him This can be explained as a result of the addition of the natural convection generated by the subject to the currents drifting through the booth In forced air flow the speed of air movement was greater without the subject than with him in the chair This was explained as a result of the obstruction the subject offered to the forced movement of air They decided that the measurements obtained without the subject were the accurate ones

and therefore always used air flows obtained without the man in the booth.

Is this satisfactory? At least for the so called still air experiments? I might point out that the air flows as measured were always very low, and the highest was something like 5.9 miles per hour in certain special experiments. Most of them were one mile per hour or less.

To get an idea of how natural convection currents flow past an object, we can look at the results of Schmidt and Beckmann (12) as reproduced in Figure 67. It shows the rate of air flow (ordinate) past a heated vertical plate in the steady state. The measurements were made by means of a quartz fiber at several points horizontally distant from the plate (abscissa). Each curve represents the measurements made at a particular height above the base. The highest rate of flow occurred at some 3 mm horizontally from the plate. About 1 cm away, *the flow was almost back to zero.*

In still air experiments, it is necessary to measure the air flow very close to the object that is giving off the heat—3 mm in this instance. Certainly 6 inches or 15 cm would be too far away for measuring natural convection currents around the human body. Winslow, Gagge, and Herrington admitted this fact.

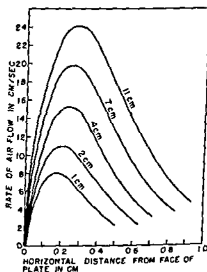


Figure 67. Relation of rates of natural convection (ordinate in cm/sec) to distance at right angles (abscissa in cm) from a vertical plate (12 x 25 cm) in the steady state. Each curve is for the given height in centimeters above the substratum. Redrawn and printed by permission from Schmidt, E., and Beckmann, W.: Das Temperatur und Geschwindigkeitsfeld vor einer Wärme abgebenden senkrechten Platte bei natürlicher Konvektion. *Tech. Mech. Thermodynamik, 1*: 341 (1930).

Burton The justification for taking the air flow a considerable distance from the man or without the man depends on the use to which this index will be put If it is to be used for the purpose of estimating the amount of cooling for a given wind velocity in climatic situations the given data on wind velocity are presumably those undisturbed by proximity to the body This is why I think the data without the man would be preferred If you are interested in the intimate physics of the surface of the body then I agree that you must know what the air flow actually is at the surface

Molnar That is true That is the practical side of measuring the air flow *It is very difficult to measure right close to the man*

Blair I think the important thing is the practicality of the studies If you are actually interested in the effect on man then you are going to have the air flow at the man

Burton Not for the purpose of translating meteorological data into stress There you want to use the standard data that the meteorologist gives you which presumably are not modified by the presence of objects close to them

Blair In order to evaluate the biological effect

Burton That would be purely academic for the purpose

Blair The practicality of what you want is the meteorological data itself and its effect on man I think that is important The data that Dr Siple has given us the wind chill graphs and charts are 100 per cent valid from the standpoint of pathological or biological effect

Burton I agree with you completely The theoretical reason I think is clear—the limiting factor producing pathology is the naked face

Blair That is correct

Burton His data should apply to unclothed parts of a man

Blair Having spent three winters in the Arctic trying to evaluate climatic stress from the standpoint of its effect on troops in the field and assuming responsibility for prevention of cold injuries I feel that Dr Siple has given us a tremendous aid in his wind chill data

In 3 years at Fort Churchill not once did I see frostbite occurring at temperatures found in the moderately cold or cold portions of the chart while such incidents were observed in the extreme cold situations The point on his chart or monograph marked where skin freezes is approximately the correct point

In January 1958 here at Fort Knox there were some 40 odd cases of cold injury during a severe cold spell and we were asked by General Ryan to prepare a directive to prevent its reoccurrence We took Dr Siple's chart modified it so as to simplify it for the field

commander, and put it out as a command directive by General Ryan We had even more severe cold, but this chart was followed rigidly and not a single case of frostbite occurred I think we shouldn't lose sight of this practical point

Burton I agree thoroughly On the other hand, if we should find out how to make a face mask which people would wear, comfortably immediately the wind chill value would not apply

Blair We don't have a face mask that anybody will wear For the time being, we have to rely on Dr Siple's data which to the Preventive Medicine Officer is all we have If we hadn't had that data available to us, I am quite sure we would have had many more cases of cold injuries in Korea

I think Dr Siple's data are of tremendous value Dr Fremont Smith brought out the point earlier about the practicality in the field of these guides, as compared to the laboratory data Here is one good chance to integrate the practical data and the laboratory data for field use

Molnar I was trying to find what the insulation of air is by looking at the procedures used by the Pierce Laboratory workers to make their measurements I agree that in a practical situation one cannot measure the flow of air right next to a man The criticism here of the Pierce Laboratory experiments, however, is that in most of them the rate of air flow was so low that it should have been measured closer to the subject than 15 cm So it seems that we cannot really accept their published measures of air flow But, since we have nothing better let's go ahead with what they did present *

As shown in Figure 68 we seek this elusive value for the insulation of air by plotting the data they tabulated in one of their papers (9) On the ordinate is the heat transfer by radiation and convection and

*EDITOR'S NOTE Dr Molnar would like to add the following "afterthought" to his remarks at the Conference

The argument that the Siple scale of wind chill should be retained despite its shaky experimental foundations because it has proved its utility in the field is to me very unconvincing for the reason that the alleged proof is difficult to find There are so many factors involved that personal experiences are not sufficient to decide the issue I know of no good objective evaluation of the empirical status of the Siple scale or for that matter of any good body of facts upon which to begin such an evaluation It may even be difficult to compile the incidence of frostbite I have seen a few cases of athlete's foot as diagnosed by a surgeon which were earlier diagnosed as frostbite by a medical officer I have also seen slightly frostbitten or nearly frostbitten ears on the first morning of a cold snap but not thereafter experience is a good teacher

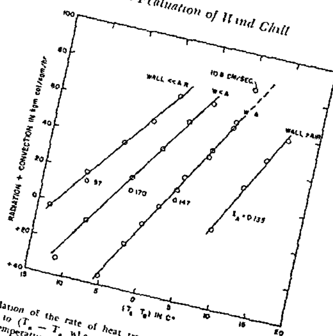


FIGURE 68 Relation of the rate of heat transfer by radiation and convection from the nude man to $(T_s - T_a)$ where T_s = mean skin surface temperature and T_a = ambient air temperature Compiled from data and printed by permission from Winslow C E A Gagge A P and Herrington L P Heat exchange and regulation in radiant environments above and below air temperature *Am J Physiol* 131 (1940)

on the abscissa is the temperature difference between skin surface and air The data fall into four groups indicated by the four curves The curves were calculated by the method of least squares This graph is not in their publication

The curve on the extreme right hand side of Figure 68 was obtained with the wall warmer than the air by 10° to 13°C For the next curve leftward the wall and air temperatures were about the same For the next curve the wall was 10 to 13°C cooler than the air Finally on the extreme left hand side the wall was 20° to 22 C cooler than the air

We find that the I_As which are the reciprocals of the slopes tended to increase as the wall got colder than the air None of these reciprocals is 0 11 although the second one 0 147 is quite close The mean of the four reciprocals is 0 162 That is still not 0 11 The value of 0 14 is for a standard rate of air flow of 10 cm /sec

In these experiments, the mean rate of flow was 7 1 cm /sec So to convert the results to the standard flow rate, the mean I_A was multiplied by 7 1 and divided by 10 The result was 0 12, which still isn t right Finally multiplying the mean I_A by the square root of 7 1 and dividing by the square root of 10 gave 0 139 So that is how we got your 0 14, Dr Burton

Burton That formula is a square root formula

Molnar That is where we have to become critical again Is the square root relationship really justified? You will notice that in these experiments the range of air flow was very small, 5 3 to 8 7 cm /sec (except for the one at 10 8 cm /sec, which we excluded from consideration because its point diverged from the trend of the other points) This is within the error of measurement and therefore representable by a mean value So these results give no indication as to whether or not the square root relationship is applicable

One justification the Pierce Laboratory workers have used for adopting the square root formula is shown in Figure 69 This is taken from the paper by Bedford and Warner (13) It shows the convective heat transfer per degree temperature difference between the surface of a sphere, either blackened or silvered, and the air plotted against the square root of the air velocity The points fall on a linear trend

Three criticisms may be made First Bedford and Warner did not

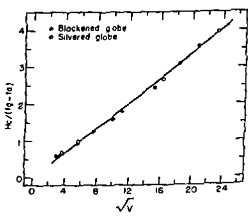


FIGURE 69 Relation of the convective heat transfer coefficient (in B t u /sq ft /hr / °F) to the square root of wind velocity (in ft /min) The globe was a 6 inch copper sphere filled with water Unsteady state experiments Reprinted by permission from Bedford T and Warner C C The globe thermometer in studies of heating and ventilation *J Hyg* 34 459 (1934)

tell how they determined the rate of heat transfer. Their globe was cooling (from about 100° to about 60°F) and therefore was not in a steady state. Second, they measured the rate of air flow with a cata thermometer which, I understand, is a questionable anemometer; about this, however, I am not fully informed and may be in error. Third, their highest rate of air flow was about 5.7 miles per hour. That is not enough to justify the extrapolation of the square root relationship to high wind velocities. In fairness to Bedford and Warner it should be noted that they used the square root relationship only because it was the simplest for the purposes of their investigation. They called attention to the fact that at low velocities the relation is not strictly linear.

With this in mind, the Pierce Laboratory workers were still not quite certain about square rooting. The sort of evidence which finally convinced them is shown in Figure 70, which is from one of their publications (14). In both graphs the ordinate is the convective heat transfer per degree temperature difference between skin surface and air. In the left graph this convective transfer is plotted directly against the air velocity. The trend of the points appears to be linear but the extrapolation intercepts the ordinate axis above the origin. In the right graph the convective transfer is plotted against the square root of air velocity. The trend is still more or less linear but this time the extrapolation passes through the origin. This passage through the origin was apparently one deciding factor for the adoption of the square root relationship (8). Actually, however, the extrapolation should not pass through the origin because even with no air movement—that is, in dead air—there is heat transfer by conduction. Also the extrapolation in the left graph is not necessarily linear to the ordinate; we do not know how that curve should go from the lowest point to the ordinate.

The upshot of all this is that there is no real factual evidence proving the square root relationship. There is also no theory necessitating it. Actually the physicists do not use the square root relationship, as can be seen by a glance at the Hilpert exponents:

Reynolds Number	Exponent
1 to 4	0.330
4 to 40	0.385
40 to 4 000	0.466
4 000 to 40 000	0.618
40 000 to 400 000	0.805

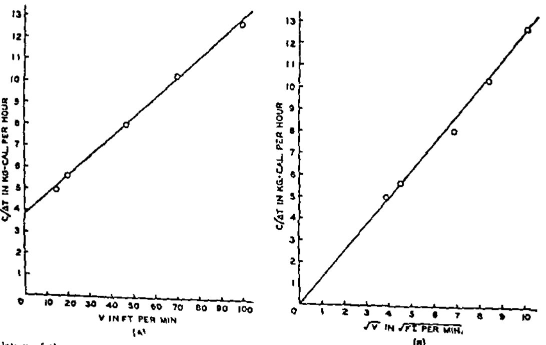


FIGURE 70 Relation of the rate of convective heat transfer per degree temperature difference between *surface and air from* the nude man to air velocity (*A*) and to the *square* root of the air velocity (*B*) Reprinted by permission, from Winslow, C E A and Herrington L P *Temperature and Human Life* Princeton N J, Princeton Univ Press 1949

For a very little low Reynolds number, *i e*, very little air movement, the exponent is 0 330 As the Reynolds number increases—that is, as the rate of air movement increases—the exponent increases until at very great Reynolds numbers, for wind of 50 miles per hour or even higher, the exponent is 0 805 The exponent for the square root is of course 0 500 Hence, we can conclude that even the experiments performed by the physicists do not justify the adoption of the square root relationship In fairness to the Pierce Laboratory workers, I should note that they did not consider that their data justified the extrapolation of the square root relationship to wind velocities above 2 miles per hour without further evidence (14) There has been no further evidence, however

Dr Burton, though, went ahead with 0 14 as the value for the insulation of still air and extrapolated square root wise Figure 71 shows where this gets us as compared with everything else The dotted curve is simply his values for the insulation of air, here expressed as heat transfer coefficients, plotted against wind velocity

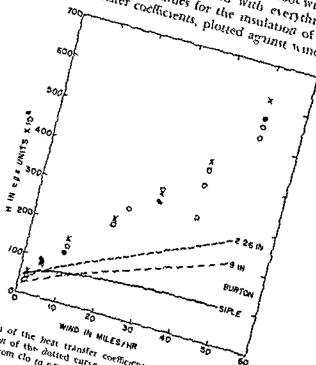


FIGURE 71 Relation of the heat transfer coefficient to wind velocity Same as Figure 69 with the addition of the dotted curve calculated as the reciprocal of Burton's I_a s (7) and converted from clo to c g s units

Burton It agrees very well with yours with the large cylinder Is that right?

Molnar It is quite close except in the beginning you are rather high by comparison

Burton In that region it is difficult to know what the wind velocity is when it is very low

Molnar No in that region the velocity is about 10 miles per hour It is only below 1 mile per hour or thereabouts that it is difficult to know what the velocity is My points start at 2 miles per hour Actually your curve should lie above the 9 inch diameter curve at all velocities because the Pierce Laboratory air flow was turbulent Would you tell us Dr Burton what you did with the copper man?

Burton When I was asked by the joint committee between the United States and Canada to establish standard values of insulation of air with wind I went through all this examination of Gagge s data and formulas I felt that in order to make it simple it would be necessary to take the power of a half *i e* the square root law Otherwise if a different power is taken for every different velocity the problem is too complicated I think the physicists data do justify as the first approximation taking the square root law

I did experiments in a cold chamber on an artificial man one made of papier mache The model was in the posture of a sitting man and I fitted it with heating coils inside and with thermostats With these I could measure the wattage the heat lost I did a series of experiments both on the ground and at altitude with different wind velocities and from these I obtained smoothed curves for the values The values I obtained agreed fairly well with those Winslow and Gagge had obtained over their more limited range (7)

Earlier in this discussion it was mentioned that at altitude the insulation of the air seemed to be better These curves do take into account the increase in insulation values at altitude They are now being rechecked by Mr Soper in Toronto and he is finding some deviation from the values I found at altitude But the values were not based merely upon the old data rather they were based on actual data on a model made in the shape of a man

I fully agree that the values need rechecking but wonder what degree of accuracy is really required for the crude purpose to which we are putting such values

Molnar I think we should all acknowledge that we can t have any exact scale of atmospheric cooling power which would apply to all conditions For a man it will vary as we know not only with the turbulence of the air and the diameter of the body but also with his

clothing posture and his activity So in a way it is committing a falsehood to use a system which implies that we are estimating the rate of heat transfer simply by combining wind and air temperature by means of a formula

Burton Do you not agree the one really big gap in our knowledge even if the knowledge is of the crudest sort—and I wish someone would undertake this—is that we have no knowledge whatever of how much the effective wind velocity is altered by the movements of a man in exercise?

Molnar Yes activity changes the effective wind velocity

Burton In other words if you have in a laboratory a man on a treadmill walking at 3 miles an hour and you know the wind velocity in the chamber is say 10 miles per hour presumably the effective wind velocity for cooling that man is more than 10 miles an hour Something must be added for his activity I don t believe we have any information

Molnar I think Belding did something on that

Horvath He made assumptions

Molnar Any sort of scale is going to be used as in the past by all kinds of people Dr Hart is a physiologist and he wants a scale to use in his study of the survival of deer calves immediately after birth We can have several possible scales just for a very simple cylinder but for something as complex as man the number of possibilities increases How shall we choose a scale? Wouldn t it be best to dissociate ourselves from the idea of rate of heat transfer as a measure of the combined effect of wind and temperature?

Carlson Buettner made measurements on the human and obtained a square root relationship In our measurements of heat loss (15) we were surprised at the increase in heat loss that movement will cause for example in bicycling there is a doubling in heat loss from the legs

Burton This is very striking in hot weather Today the temperature may be tolerable as long as you are walking around When you lie still on a bed it becomes intolerable I think it is a very important point on which we have far too few experiments

Siple This problem is very complex not only is it a question of the amount of breeze the subject contributes as he walks or moves around through the air but also it is a question of the air movement within the clothing itself which requires another complete series of considerations and analysis

Horvath On the nude man the square root factor does work fairly well at least in hot environments

Fremont Smith I wonder if we can't learn something by watching the position of animals exposed to heat and cold They seem to be quite aware of the effectiveness of drawing themselves together into a ball when it is cold and extending themselves when it is hot

Blair Richet (16) wrote quite a bit on the animal's behavior in response to heat or cold as shown by its body position

Siple The majority of them are clever enough to go in out of the cold and wind They come out voluntarily usually when it is calm

Fremont Smith They are quite resourceful about doing something I want to call attention to that smartness it has had a long evolution

Molnar Earlier this afternoon we saw that the thermal analogue of electrical power was this expression $AH\ (T_s - T_a)$ Let's pattern a similar expression for wind chill

$$WC = kVT_a^2$$

We shall retain Dr Siple's term, wind chill We make it equal to the product of a constant k, wind velocity V, and the square of air temperature For convenience, we can use the wind velocity measured at the nearest meteorological station, its units would be miles per hour Instead of a temperature difference, we can use only the air temperature because surface temperature varies considerably is usually not conveniently available, and, for a steady state expression would have to be arbitrarily fixed Actually, however, I am arbitrary in suggesting that T_a should be not air temperature as such but the temperature of air below some biologically critical value say that of freezing We would forget about everything above 0°C our wind chill would apply only below this temperature

In accordance with the analogy, T_a is squared This is not the same as taking the square root of wind velocity, for to obtain this the above expression would become

$$\sqrt{WC} = \sqrt{kV} \cdot T_a$$

that is the square root of wind chill would be proportional to the square root of wind velocity

Again according to the analogue, it is permissible to have a coefficient Let's arbitrarily have a coefficient $k = 0.01$ to make the value of wind chill small

As an example if the wind is blowing 10 miles per hour and the air temperature is −10°C, then

$$WC = 0.01 \times 10 \times (-40)$$
$$= 0.01 \times 10 \times 1600$$
$$= 0.01 \times 16000$$
$$= 160 \text{ units of wind chill}$$

That may seem strange but when the history of thermometry is recalled we are reminded that similar problems existed in the past. What should a unit be? For temperature it was called a "degree." What is a degree? On the centigrade scale it was taken to be not a certain number of millimeters on the stem of the thermometer but one hundredth of the distance between the boiling point and freezing point of water whatever this distance may be in millimeters. This was arbitrary so if it was acceptable for our ancestors to be arbitrary then perhaps we can be arbitrary here with units for wind chill.

As another example let the wind velocity be 50 mph and the air temperature only −20°C. Then

$$WC = 0.01 \times 50 \times (-20)$$
$$= 0.01 \times 50 \times 400$$
$$= 0.01 \times 20000$$
$$= 200$$

Burton Is this 0°F as your critical temperature?

Molnar No, 0°C.

Burton Why choose this? That means there is no wind chill if it freezing at that temperature wind has no effect at all.

Molnar That is one of the absurdities.

Burton Are you presenting this as something empirical or as logical?

Molnar I am presenting this as something to fill the void I left at the end of the dry run.

Siple Which it is not possible to check experimentally.

Molnar Checking isn't necessary; in fact it can't be checked because it is just an arbitrary combination of wind velocity and air temperature in correspondence with the analogue of electrical power. What is needed is a determination of the empirical correlation of biological variables such as tolerance to cold, incidence of frostbite, etc. with this scale of wind chill.

Siple Does this follow? Have you plotted enough of these figures by this system to derive a family of curves?

Molnar I calculated a table but I did not plot it.

Siple Let me explain a bit concerning the origin of my work on

wind chill My first empirical version was developed during 1938 and 1939 I started off, as you have done, by using the idea of multiplying the temperature times the wind velocity, and I was using Centigrade degrees below zero, as you have I did not square it For wind speeds, I used a scale in meters per second *

When we arrived at the Antarctic in 1940, we tried to apply my empirical formula but we found it bore little relationship to the comparative sensation of cold When we went out in the cold, with the high wind velocity, it obviously wasn't as much colder than at moderate winds, as the empirical numbers suggested The squaring factor in your empirical formula may improve your curve over that of my early empirical one

Molnar Actually I thought of squaring the air temperature first and then I back tracked to the electrical analogy to let it be my justification

To answer Dr Burton's question, I felt that problem could be taken care of by just adding T_a, or

$$WC = kVT_a^2 + T_a$$

This would give a value for wind chill even when the air flow was zero But this is not good enough because at 0°C it would still give 0 for wind chill So let's take an arbitrary air temperature of +30°C and subtract from it the ambient air temperature, or

$$WC = kV(30 - T_a)^2 + T_a$$

This would always give a value for wind chill I found, however, that for really significant combinations of wind and temperature, this elaboration made little difference

Burton It is very easy to get frostbite with just a few degrees below zero, if there is enough wind So, your formula would be very inadequate when you get up toward freezing

Blair Frostbite can be caused by a temperature of −2°C and a 30 mph wind

Burton I think you should choose your critical point considerably higher than freezing temperature.

Molnar That is all right I don't care where you choose it

Carlson You are putting a lot of emphasis on freezing Wind chill is a kind of cooling power, and it applies above zero as well as below

* Carslaw and Jaeger (17) have presented another theoretical foundation for a scale of wind chill based on the rate of cooling It has not been subjected to experimental verification

Blair We should never forget that temperature well above freezing cannot produce frostbite but can produce trench foot or immersion foot which is just as important

Molnar You don't have to start at 0°C You can start anywhere else

Burton I would make another criticism of your empirical formula The fact is that the big difference in wind chill does occur between still air and 5 or 10 miles per hour After that an extra increase of wind velocity has little added effect whereas your formula is proportional to the wind velocity

Molnar I think that the data in Figure 71 disprove that Dr Burton

Burton My statement is based on empirical data on empirical facts

Molnar I don't know what your facts are but the ones we can look at are on Figure 71 Even the Hilpert curve shows practically a linear increase in the heat transfer coefficient with increase in velocity If for the moment you will trust our measurements and assume they were made in turbulent flow we see that they too show a linear upward trend without leveling off to a horizontal parallel with the abscissa

Burton Our difference in point of view is that you are still trying to predict a heat loss

Molnar I am trying to get away from heat loss

Burton What is this empirical index? What is the meaning of units containing the square of the temperature? It can't be heat loss can it? What is it?

Molnar If reduced to this simple expression

$$WC = kVT_a^2$$

the dimensions are miles per hour air temperature squared

Burton Yes but physically if you say this is giving an index of heat loss the units have to be of the right dimensions

Molnar I am not saying that I am saying get away from heat loss from heat transfer as our index

Davis Isn't there enough data on wind and temperature on frostbite

Blum Dr Siple has that You have wind and temperature

Davis You return to Dr Siple's curves

Blum Yes but actually as far as field application you have as good a curve as you need for determining susceptibility to frostbite and under what conditions frostbite occurs

Davis You could have a good curve without the Siple curve

Barquist It is duration of exposure at different levels of wind chill that we are working with now, isn t it?

Siple I would like to congratulate Dr Molnar on the analysis he has made from a technical standpoint I thought I could name all of the possible criticisms of wind chill however his examinations of details of heat loss I find fascinating he gives as well the actual laminar heat losses and so on I think his work is excellent

Of course our original work in 1940 was limited by the fact that it had to be done in the open ambient atmosphere We did not have laboratory facilities We had only a left over thermometer available from a glacial study program that we were able to convert into the device we used We even had to make the cylinder from flat plastic all of which was an achievement at the time and under the conditions of 18 years ago McAdams book was not available We had virtually no reference books in fact But on the other hand having access to cooling tables might even have inhibited our attempting the experimentation we did try to carry out

These remarks are not intended as defense but rather explanation to the group here Of course when you get something into literature once you have to live with it For the past 10 years or so I have been recommending that my wind chill formula be considered simply as an empirical table Although the numbers are labeled as key calories per square meter per hour they should more appropriately be used as Dr Molnar suggested just as numbers

The only thing that I question about Dr Molnar s figures is that he may have produced a misshapen curve in respect to the sensation of cooling Perhaps it fits closer than I believe at the moment

The tables and nomograms we made up with the wind-chill data have worked out remarkably well in practice out in the field As an empirical set of numbers the shape of our curve has followed the sensations of chill by many who have put it to practical use I fully recognize the limitations of my physical experimentation because of the particular set of circumstances under which it was developed I have always hoped perhaps that someone would devise some sort of small instrument that we could both measure and check on a simple empirical formula There is perhaps nothing wrong with a nomogram I think the success of my wind chill formula has been largely due to the original nomogram No one ever calculates the wind chill from the formula because it is too much trouble for the average user to make such calculations With a nomogram the value

can be picked off the chart simply by entering with the temperature and wind

We advisedly used plastic rather than a tin can for our cylinder I could have used a tin can In fact this would have been much easier to adapt However the reason we used plastic is because we were trying to simulate with materials at hand something that was nearer to what we suspected would be the flow of heat through human skin We thought plastic was more like skin than metal however we were fully aware of the fact that metal would have conducted heat faster We had no means at hand for stirring the water in our cylinder and this worried us I did experiments later and realized I could have hastened the speed of heat loss greatly by stirring

Again I felt perhaps the actual heat loss from the body should be lessened because of a slower conductivity through the skin

Dr Burton and I talked a good many times about the fact that my formula was for a nude man I agree my original formula was rather impractical because a man wouldn t be naked in the cold However sensing cold is usually achieved by either the bare hands or the face Although my formula does measure actual losses of body heat it does reflect the intensity of the feeling of cold

I have always felt that the meteorologists and the physicists who design their instruments have been slow in devising an acceptable meteorological instrument for measuring wind chill Of course calculations from a table may be adequate There are a great many needs for a device that can measure an index approximating the cooling sensations experienced by human beings I know that from a practical standpoint we will never find a practical formula or instrument no matter how refined which can serve as an accurate measurement of human heat loss

I remember Colonel Hatch made one of the most complete cooling formulas during the war which as I recall had about fifteen or twenty different equations and variables It proved to us conclusively that we could never devise anything that was going to be practical because it took an hour to work the formula and it took only a second or so to change the condition of exposure

I agree that perhaps we should not assign even by implication as I did actual amounts of heat loss in kilogram calories per square meter per hour This is misleading but when we did our work we just didn t know this

You have proven that we should not try to produce wind-chill formulas with positive values unless we can come to agreement on some standard figure or shape

I have visualized an instrument in the form of small 1- or 2-inch high cylinders or spheres having no direct relationship to the human body, but at least a physical inanimate form for which the cooling characteristics are known It would be possible to set up such an instrument within a physiological laboratory, and get an integrated measurement of the combinations of air movement, conduction, radiation, and evaporation The values of each could be read separately or in combination Perhaps some of the hot wire anemometer devices do this to an extent

I fear that I am skeptical about the particular solution you suggest Considering the detail into which you have gone, I would like to suggest that perhaps you could go a little further in your laboratory experiment and devise some standard, simple instrument which could be used in other laboratories and in the field You could then establish your own formula to fit the cooling curve of the instrument It doesn't matter, as long as the curve is in the general neighborhood of human sensitivity and reproducible Then I think we would have something on which we could standardize

I would prefer to see a wind chill standard based on a reliable meteorological instrumentation than on the crude cylinder and methods we used 18 years ago Based on the things you now know perhaps we can make one, and one which might be a family of heated small, geometric bodies, one sheltered from air movements the others not They could be exposed further to radiation, shade, with dry or wetted surfaces This combination would then permit selections of values out from totals and combinations The instrument might read in electrical units instead of calories that is, it might read in emf or wattage

During the late period of World War II, Plummer made an elaborate device for measuring cooling It was so sensitive that just the slightest motion of air going past it would make an instantaneous response, turning an internal heating mechanism This device was far too elaborate and expensive I think what we really need is something that is simple, although it might well have a method of automatic recording

Dr Molnar, I was fascinated by your meticulous analysis of my crude wind chill experiments You supplied answers to questions we had wondered about but were unable to study at the time Looking back we perhaps made a rather too naive approach, and we may have made assumptions which were a little careless However from the practical standpoint I think we evolved a schema that has been of some use

Burton: Many other people including Bazett and me made such an instrument. But the great difficulty about having it small and portable is that unless it is at least 7 inches in diameter there will be this curvature effect which means that the way the readings are affected by the wind is very different from that for a larger cylinder and for man. But an instrument that is either a globe or a cylinder 7 inches in diameter or greater is not easily transported.

Siple: This is true primarily. The Reynolds number and cylinder shapes that Hatch and Plummer (3) devised were an indefinitely long cylinder—9 inches in diameter for dry cooling equivalent of a man and 8 inches in diameter for a wet or sweating man.

Burton: During the war I used to have such an instrument, a very simple one, and I used it a lot.

Siple: I agree that a more appropriate shape would come closer to fitting the shape of the cooling curve to the cooling rate of the body. Some people who have been interested in wind cooling of buildings, engines, and such heated items have tried to use the wind-chill formula, and despite the great deviation in the Reynolds number they have derived some usefulness from it.

Possibly if the instrument could be made of a small practical size and accepted as just a reproducible curve, corrections could be applied for various applications.

Blair: During the war the Japanese in Manchuria had a rather unique instrument. They called it a freeze meter. It was so simple that its practicality was often questioned. It was like an hourglass containing a fluid the viscosity of which changed as it cooled.

They kept it at body temperature next to the body, then exposed it to the air and read off whether they could get frostbite. It showed how long a man could stay in the cold safely. It seemed to work in the cold quite well but how effectively I don't know.

Molnar: The Davos frigorimeter is another instrument for measuring and integrating the thermal variables of the environment (18, 19).

Irving: In the interest of simplifying your expressions, you could use the Beaufort wind scale. You are looking for dimensions which can be estimated without instrumentation. The commander of a small unit in the field is not going to have an anemometer. The Beaufort scale has been widely used.

Molnar: Whether rate of heat transfer or rate of cooling should be used for a scale of atmospheric cooling power is a problem for further thought. From the point of view of exact science, a unitary scale applicable to many conditions is an impossibility. On the other

hand from a practical point of view multiple scales would be cumbersome confusing and perhaps worse than none at all When and if a scale is finally adopted its limitations should always be clearly indicated Whatever the scale its limited usefulness will require establishment by empirical correlations with biological variables If these cannot be performed by critical mentalities it would perhaps be better for them not to be performed at all

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SHIVERING AND NONSHIVERING HEAT PRODUCTION IN ANIMALS AND MAN

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THIS PROBLEM HAS BEEN ONE of long duration At least 90 years ago Claude Bernard (1) using curare and spinal section pointed out that during cold exposure the possibility of another source of heat production in addition to shivering might be present After him Rubner (2) Lefevre (3) and Cannon (4) felt that this was true but it was very difficult to demonstrate Many have attempted to demonstrate this nonshivering thermogenesis which is by the way Dr Keller's terminology And consistent failure to demonstrate nonshivering thermogenesis has led many to believe that it does not exist Therefore it appeared that shivering was the only method of heat production in the cold This is the easy way out but it is not satisfying

It is difficult to deal with this problem without resolving some of the problems of shivering To measure shivering to determine its contribution to heat production in the cold has also presented problems With shivering the difficulty has been in deciding its mechanisms of regulation and just what its relationship is to core temperature and skin temperature

Certainly infusion experiments show that if the brain temperature is lowered shivering can be stimulated If the brain temperature is raised shivering can be inhibited This led many investigators to consider that perhaps shivering is regulated by changes of central temperature This does not happen under all situations Within the physiologic range of normal temperature we can elicit shivering by skin temperature changes which seem to play a major role under these conditions In this presentation an attempt will be made to clarify and unravel some of the problems involved

Table III demonstrates the sensitivity of obese hyperglycemic

mice to cold (3°C) as compared to their nonobese siblings and goldthioglucose obese siblings (5). The nonobese siblings appear to be perfectly normal when examined from every available parameter. The goldthioglucose obese animals are nonobese siblings which have been made obese by the administration of an I.D.$_{50}$ dose of goldthioglucose. The obesity so obtained is comparable to the obesity of the hyperglycemic mice. The hyperglycemic mice are characterized by a measurable hyperglycemia associated with a high degree of resistance to insulin. Endocrinologically, these animals appear to be normal except for a marked degree of hyperplasia of the islets of Langerhans (6). Table III shows a mean survival time of 2.2 hours for the obese mice, whereas the nonobese and goldthioglucose obese animals are still alive after an exposure period of 3 days, indicating the extreme sensitivity of these animals to cold.

TABLE III

Effect of Exposure to Cold on the Duration of Survival in the Hereditary Obese Hyperglycemic Mice and Controls

Animals	Nonobese	Goldthioglucose	Obese Hyperglycemic
	12	12	21
Duration of survival	> 3 days	> 3 days	2.2 hours
Range	—	—	0.5 to 5.5

Reprinted by permission from Davis, T. R. A., and Mayer, J.: Imperfect homeothermia in the hereditary obese hyperglycemia syndrome of mice. *Am. J. Physiol.*, *177* ... (1954).

Keller: What was the ambient temperature?

Davis: It was at 3° to 5°C. Another characteristic of the obese hyperglycemic mice is that after considerable starvation, they utilize their protein without utilizing their fat storage. They appear to be quite unable to mobilize their fat for fuel purposes.

Figure 72 shows the fall in rectal temperature and oxygen consumption during exposure to cold (5). For our purposes we will disregard the accompanying change shown for respiratory rate. The top curve is colonic temperature and the bottom curve is oxygen consumption obtained by the Haldane method. The ordinate is the exposure time in hours. The particular results shown in Figure 72 were obtained from one mouse but are representative of the behavior of ten animals. The colonic temperature shows a progressive

fall until death supervenes after 4 hours of exposure, with a body temperature of 14°C During this fall in body temperature the animals respond with marked shivering and pilo-erection Vasoconstriction is also in evidence as determined by skin temperature change In other words they do all those things which are deemed necessary to resist cold However, they go into hypothermia and die, indicating that it takes more than the presence of marked shivering pilo-erection and vasoconstriction to resist hypothermia

Henschel It is interesting that oxygen consumption also decreases with shivering Apparently shivering was not taking place at the time you started zero time but your oxygen consumption continu

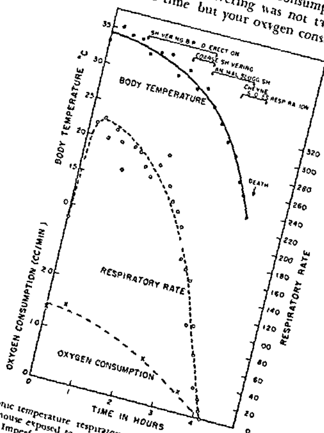


FIGURE 72 Colonic temperature respiratory rate and oxygen consumption of an obese hyperglycemic mouse exposed to cold Reprinted by permission from Davis T R A and Mayer J Imperfect homeothermia in the hereditary obese hyperglycemic syndrome of mice *Am J Physiol* *177* 222 (1954)

ously goes down even though shivering takes place This is a case of obtaining a decrease in oxygen consumption with muscular activity

Davis This is true However, if I may draw an analogy here this is a little like a hill that can be climbed only by a 100 hp car If the car has only 90 hp it will run backward and the 90 hp will be masked

Horvath Are the four points the only points you measured? You can t make any determination as to the reliability of the curve You don t know about the oxygen consumption Why have you drawn this curve? Is this interpolated?

Davis This is interpolated We don t know the oxygen consumption in between but with the points we do have it appears as though the animal did not respond by an increased oxygen consumption

Fremont Smith By the time you got the second point his body temperature has also gone down already So I think Dr Horvath is quite right we missed the crucial point in terms of oxygen consumption

Davis Yes

Burton Is your prejudice toward believing you have an increased shivering without increase of oxygen consumption? It would be a very strange thing to believe unless one had the definite evidence for it

Davis I am not trying to believe or disbelieve it however I admit I am prejudiced

Burton I am trying to rearrange your prejudices

Davis I think the point we should remember as Dr Fremont Smith has mentioned is that we already had a degree of hypothermia By the time we had obtained these points hypothermia had taken place These animals go into hypothermia as soon as they are placed in the cold There appears to be no response

Horvath The temperature is roughly around 30°C at the time you have them marked and I would say quite near 50 per cent reduction in metabolism

Hart I think the point is that we do not know the normal metabolism when they are not in the cold

Davis The normal metabolism of these animals is 1 4 ml of oxygen per minute and is the mean figure of many animals

Hart Was it 0 5 ml at normal temperature?

Horvath He says his normal is 1 4 ml

Hart Then the metabolism was not elevated

Davis It is possible to obtain figures in the cold before the deep

hypothermia as Dr Horvath is pointing out You can get the metabolism up to about 1 6 1 8 ml per minute In this case it has only gone up to 1 5 ml per minute If the body temperature of these animals is maintained with diathermy there will be a considerable rise in oxygen consumption in other words diathermy unmasks what capability they have to produce heat

Fremont Smith If the body temperature had been maintained in the presence of a cold environment and shivering then the inevitable conclusion would have been that the oxygen consumption would have increased

Davis Yes

Fremont Smith Therefore the fact that the body temperature has gone down here is consistent with the decrease in oxygen consumption

Davis If the body temperature of these animals is maintained with diathermy in the cold a considerable rise in oxygen consumption is obtained In other words you are unmasking what is existing by maintaining the body temperature

Fremont Smith Are you perhaps saying that the muscles that are shivering are showing an increase in oxygen consumption but that the body core as a whole with its drop in temperature has such reduction in metabolism that the reduction in oxygen consumption in the rest of the body masks the increase localized in the shivering muscles?

Davis Yes

Burch I don t think you have done that Have you actually measured the shivering?

Davis That animal is visibly shivering

Burch The temporal relationship of the two points is not evident

Horvath Although you have drawn a smooth curve I don t know whether you are quite justified You start off with the initial temperature around 33 5°C go up to nearly 35 5°C and ignore it Is that not an increase in temperature?

Davis I don t think it is ignored if it is on there

Horvath You have ignored it by the curve you have drawn

Davis I have simplified it

Keller Does this represent an algebraic mean of several animals or individual animals?

Davis There were about 12 done Figure 72 shows one of them

Keller This is an individual protocol?

Davis This is an individual

TABLE IV

Effect of Various Treatments on the Duration of Survival in the Cold of Hereditarily Obese Hyperglycemic Mice*

Treatment	None	Thyroxin (25 μg/day)	Thyroxine (100 μg/day)	Thyrotropic hormone (25 mg/day)	2 4 Dinitro phenol (25 mg/day)	ACTH (25 mg/day)
Number of obese mice	21	4	4	4	4	4
Average duration of survival and actual values	2 2 (0 5 1 0 1 5)	3 [illegible] (3 3 3 5)	14 3 (1 12 17 24)	6 5 (4.5 6 5 7 8)	3.5 (3 3 4 4)	0 8 (0 3 0 7 0 9 1 3)

*All treatments administered for 48 hours before exposure

Reprinted by permission from Mayer J and Barnett R J Sensitivity to cold in the hereditary obese hyperglycemic syndrome of mice *Yale J Biol & Med* 26 38 (1953)

Keller What was the variation between animals?

Davis The variation was from half an hour to 6 hours of exposure at 3°C to produce death

Keller I mean in rate of cooling

Davis The rate of cooling here is 4 hours it is slower in some and faster in others

Keller They are not uniform then?

Davis They are not uniform

Taylor Is it possible to energize shivering by anaerobic glycolysis?

Davis I cannot answer that Can someone else?

Henschel Would you expect anaerobic glycolysis to go on in this case? Shivering continues for 2 hours or more Isn t that long?

Carlson You don t get lactic acid

Taylor In shivering?

Carlson No

Taylor I was wondering about this particular preparation

Carlson I can t answer that

Bass What about initial rectal temperature?

Davis These animals maintain a rectal temperature of 34° to 36 5 C in a reasonable room temperature of 26°C

Bass Was that animal hypothermic to begin with?

Davis By normal standards yes

Burton These are a genetic mutation strain of mice?

Davis That is right

Keller There is considerable variation among those?

Davis Yes Table IV shows the effect of various treatments on the survival in the cold of obese hyperglycemic mice The results in this table were obtained by Barnett (7) who was working in conjunction with us He investigated thermogenic agents believed to increase resistance to cold only thyroxin increased the survival time to 14 3 hours and relatively large doses had to be used to achieve this I do not think the other figures are significant since there were not enough animals in each group But the 14 3 hours is significant

We concluded that the physical characteristics play no role in this increased sensitivity to cold because control animals of similar weight and a similar degree of fatness were resistant Therefore increased sensitivity was probably due to a failure of chemical means of heat production rather than a failure of mechanisms regulating heat loss and it would appear that some heat production mechanism in addition to shivering must exist (5)

Figure 73 records our attempts to resolve the question of whether

shivering was dependent upon changes in colonic temperature or upon changes in skin temperature (8) The top part of the ordinate is shivering activity expressed as units of electrical activity of shivering obtained by integrating muscle action potentials recorded with an EMG The bottom part of the ordinate is temperature in degrees centigrade The abscissa is exposure time in minutes The exposure temperature used was 5°C The top curve is shivering activity, the next curve down is colonic temperature, the third curve is body skin temperature, and the lowest curve is tail skin temperature The tail skin temperature was obtained at the bare area between the end of the hair line at the root of the tail and the beginning of the horny character of the tail

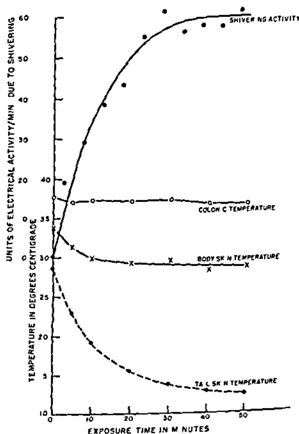


FIGURE 73 Curves of body temperatures skin temperatures and shivering activity in shaved rats exposed to cold (5°C) Reprinted by permission from Davis T R A and Mayer J Nature of the physiological stimulus for shivering *Am J Physiol* 181 669 (1955)

The shivering curve can be seen to be a mirror image of the tail skin temperature curve with a correlation of better than 0.9. The correlation with colonic temperature was only 0.03 and with body skin temperature it was 0.34. This would indicate that at least in this situation the shivering activity is correlated very highly with changing skin temperature.

Fremont-Smith: The temperature of a patient who has been given typhoid vaccine intravenously and who is just between spasms of shivering is rising but the patient does not shiver at the moment he pulls a blanket over himself in order to keep warm because he is so cold. His skin temperature is dropping, the blood flow through the skin of his fingers has dropped and there is a spasm of the arterioles.

If the blanket is raised once and lowered, making a breeze of the ambient room temperature on his skin, the patient is thrown into a spasm of violent shivering. I think this is confirmation, at least for the human being. I have done it six or eight times with patients who had been given typhoid vaccine.

Davis: This also holds true for typhoid and malaria patients.

Henschel: Did you measure skin temperature in any other place?

Davis: We did paw temperatures and found they were close.

Henschel: What about other places on the body besides the tail?

Davis: We have body skin temperature but it was measured only in one spot which was on the flank. In humans the temperature of the skin of the trunk also follows this pattern.

Taylor: When the shivering activity curve stops ascending does that mean the shivering has stopped?

Davis: No. Shivering is at the highest peak of activity. As you can see in the shaved rats shivering started immediately but it did not become apparent until after 15 minutes of cold exposure. In other words, you can see shivering visually only after a certain degree of activity has been reached. Prior to this, characteristic bursts of activity can be picked up by the EMG between zero time and 15 minutes.

Carlson: It is interesting that you have a high shivering activity when skin temperature is changing little. In terms of rate of change the initiation of shivering lags behind the drop in skin temperature. The maintenance of shivering with little change in skin temperature is difficult to explain.

Davis: This is an even worse problem in the human.

Horvath: Why did you stop at 50 minutes?

Davis: We just made an arbitrary stop at 50 minutes.

Horvath That leaves you with the V

Davis I might mention that in this situation curves of high and low activity are obtained This constantly changing activity seems unassociated with changing oxygen consumption it is entirely un associated with any apparent change in skin or colonic temperature

Keller I would like to comment on Dr Carlson s remark An ex planation of that problem would be either that shivering is being driven by slow adapting receptors or slow adapting central neurons which would continue to discharge without a change in strength of stimulus

Carlson If you accept Hensel s (9) data on sensory endings the adaptation

Keller T

Burton ‘ ors of the tongue and I am at a loss to see how that is related to tempera ture regulation except for protecting the tongue from burning Don t the data show they are not complete adapting receptors? They do continue to fire even though the temperature is not chang ing any more The rate of change does come in but they do continue to fire even though the temperature is kept at a constant level

Carlson Hensel has some data on skin (9) The main point is that with a rapid rate of change of temperature the highest frequency output (9) is obtained As absolute temperature sensors cold re ceptors cover a sort of frequency spectrum (9) some firing maxi mally at one temperature others at another The maximum impulse frequency would occur during a changing temperature

Davis If anyone has doubts about the EMG we have tried to ob tain an index of shivering activity by counting bursts and expressing results as number of bursts per minute We obtained a curve slightly to the right of the one depicted here We were not satisfied just to use frequency since activity at any time is a product of the amplitude and the frequency of its occurrence It is a tedious chore to measure the amplitude at every point and multiply by the time of its occur rence although an integrated curve can be obtained this way

Figure 74 gives further evidence of the dependency of the develop ment of shivering upon skin temperature and shows the effect of skin insulation upon the development of shivering The ordinate is units of shivering activity The abscissa is exposure time in minutes The top curve shows the development of shivering in shaved rats the next lower curve shows the same in unshaved rats and the bottom curve is shivering activity of unshaved rats with tails protected from the cold with absorbent cotton

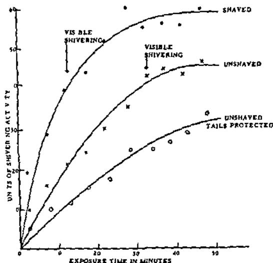


FIGURE 74 Curves of shivering activity in the cold (5°C) in rats (ten animals) shaved unshaved and unshaved with tails protected Reprinted by permission from Davis T R A and Mayer J Nature of the physiological stimulus for shivering *Am J Physiol* 181 669 (1955)

In spite of the variability that exists from rat to rat the differences among the three groups at the end of 50 minutes were significant There were ten animals in each group In the shaved and unshaved group visible shivering occurred During the period of exposure shivering was not observed in the unshaved rats with the tails protected (8)

Horvath Did you cut off the tails of any of these rats?

Davis We thought of cutting the tails off and also of producing a nerve block of the tail but we did not do it In the rat we were able to obtain typical EMG shivering records without being able to see the animals visibly shivering In the human being and possibly in Dr Keller s dogs the beginning of visible shivering or conscious knowledge of it by the subject or EMG evidence of shivering usually occurred at approximately the same time It is therefore possible that in the dog the appearance of visible shivering also marks the actual beginning of shivering This may be due to nothing *more than the size of the animal In this particular instance (Figure* 74) it does appear that physiologic shivering does depend on skin temperature A shivering level is maintained even though it is maintained in waves of activity separated by periods of quiescence

Fremont Smith On change in skin temperature?

Davis Certainly a lowering in skin temperature A shivering level is maintained even though it is maintained in waves of activity separated by periods of quiescence

Figure 75 shows the effect of diathermy on rectal temperature skin temperature, and oxygen consumption of rats exposed to cold The abscissa is intensity as measured by watts input to the final stage of the radio frequency generator The ordinate on the left side is rectal temperature and oxygen consumption expressed as per cent of the cold induced metabolism On the right side of the ordinate is skin temperature in degrees centigrade As diathermy intensity is increased, we find that the rectal temperature starts to change after about 45 watts (10)

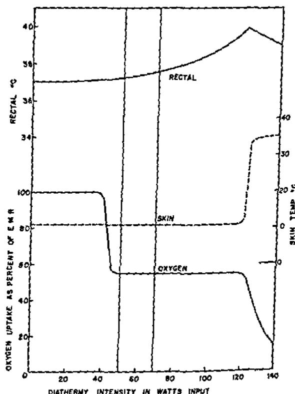


Figure 75 The effect of diathermy intensity upon rectal temperature skin temperature and oxygen consumption Reprinted by permission from Davis T R A Johnston D R and Bell F C The regulation of shivering and non shivering heat production during acclimation of rats *Report 361 U S Army Med Res Lab* Ft Knox Ky 1958

Burton May I ask the time scale? Is this after a steady state has been reached or after what time?

Davis In this study, we give diathermy for 10 minutes at increments of approximately 10 watts In all studies where we used diathermy, the period of application was usually 10 to 15 minutes

Burton Is it at the end of 10 minutes that you record how much it has risen?

Davis Yes

Burton The temperature is still changing, probably

Davis Yes, the effect does depend on duration of application in so far as colonic temperature is concerned This is not true in so far as oxygen consumption depression is concerned Since depression is obtained almost immediately after the beginning of the application and remains in the new level provided, diathermy does not raise rectal temperature to an excessive degree

Hart Is this a single animal?

Davis No this is a diagrammatic representation of something like twelve animals I think there were more

Horvath What kind of diathermy was used, long wave, short wave?

Davis This was 27 21 megacycles We kept in a band allowable for physiotherapeutic diathermy We chose 27 21 megacycles for the very reason that we knew it penetrated well and could give us our response It was the first one we tried, and it worked We know dia

of this artificial heat production the rectal temperature falls

Burton Could I ask what this intensity means? This is no measure of the heat you are actually delivering to the animal It is some sort of calibration of your diagram

Davis This is a measure

Burton You are actually producing 120 watts to a mouse?

Davis No I will explain this watts input I put the word in put —

Burton This is the calibration of the machine, not the dose of heat given to the rat?

Davis No it isn t This is calibration of the machine As a matter of fact this is under our very specific conditions If you change the conditions slightly you may have to go up to 500 watts In other words it depends on where the animal is in relation to the irradiating surfaces

Burton Do you know how many watts the animal is actually getting?

Davis No

Burton Probably 1 or 2 watts?

Davis We generally believe that the watts input as compared to the watts output has an efficiency of 75 per cent But this depends of course, on the position of the animal and the distance of the plates from the animal since diathermy behaves in accordance with the inverse square law

The effect on the skin temperature is somewhat different Exposure to a temperature of 5°C produces a skin temperature of around 11°C As the intensity of diathermy is increased, we find that skin temperature remains at low levels until 120 watts are reached or at that point where rectal temperature in the upper curve shows a fall At this point, skin temperature rises sharply to somewhere in the vicinity of central temperature indicating an active vasodilation This is accompanied by a marked increase in heat loss to the 5°C environment This, of course, explains the fall in rectal temperature shown in the top curve

I think some of us will recognize this as at least part of a process that occurs in malarial fever, or in your experiments except we produce lowering of skin temperature by cold exposure The oxygen consumption curve shows a fairly dramatic fall at around 40 watts of diathermic intensity to about a level of 56 per cent of the total cold induced metabolism, remaining at this new level until 120 watts are reached, where it shows a tendency to fall toward basal levels At this point, I should explain the meaning of EMR This is environmental metabolic rate which I define as the metabolic rate above basal metabolic rate produced by thermal stress The zero in Figure 75 is, therefore, the basal level

I think I should mention here that, to date all attempts to show that diathermy has effects other than purely thermal ones have failed In our own experiments, we find that the intensity which will produce death in 10 minutes at room temperature will have no effect on the animal in the cold, indicating that death was indeed due to hyperthermia

Burton Is this final change on your graph always irreversible?

Davis Not always

Burton Do this sudden rise of skin temperature and drop of oxygen consumption represent a terminal event, death?

Davis No this is not death The drop of oxygen consumption is

not always seen with this dilution but the rise in skin temperature is always reproducible and reversible. This is in the cold.

Burton: Is it reversible, I mean? Can one stop the diathermy and bring about recovery of the animal?

Davis: Yes. It is reversible: if the diathermy is stopped, the skin and rectal temperatures will return to pre-application levels and oxygen consumption will increase again.

Our interpretation of these results is that between 45 and 120 watts the fall in oxygen consumption is the result of either increased rectal temperature or increased body heat content. It does not fall any farther because skin temperature remains unaltered and is stimulating that part of metabolism which is dependent upon changes in skin temperature.

We therefore think that we have substituted by diathermy that part of heat production which is sensitive to changes in core temperature and the part that we could not change within that range could not be changed because the skin temperature was still at low levels. In other words, this is the part of heat production which is dependent upon skin temperature stimulation. It would therefore appear that we had in our hands a desirable tool for the investigation of temperature regulation in the cold, and that if we chose a range of diathermic intensity between 50 and 70 watts we would be able to carry out investigations which might clarify the subject. This range of 50 to 70 watts under our conditions of experimentation is chosen because rectal temperature is not increased too greatly, skin temperature is unaffected by the diathermy, and finally a maximum and stable depression in oxygen consumption is produced.

Burch: Were the animals shaven?

Davis: No.

Burch: Was there visible vasodilation in the skin when the skin temperature went up?

Davis: Yes, you could feel it and you could see the tremendous change in chamber temperature immediately.

Burch: What about the tail?

Davis: The tail became very hot. In fact, that is where you could feel the heat, right along the tail, not just at the root.

Burch: The amount of heat generated internally became a heavy heat load to lose.

Davis: I think this is what happens. This is no more than a repeat of two other experiments, one that Hardy (11) carried out and one of Barbour's (12) infusion experiments. In other words, if the central

temperature is raised high enough *the heat load is such that it must* be dissipated This is done by reflex vasodilation

Burch A moment ago you stated the skin temperature was the stimulus for heat production It appears to me that the skin temperature increased secondarily Internal phenomena changed the skin temperature and the rates of metabolism and oxygen consumption Skin temperature may have had nothing to do directly with oxygen consumption

Davis *I am not talking about that part of the graph* I was talking about an earlier part of the work where we chose 50 to 70 watts for critical investigation because the skin temperature was not changed

Hart Skin temperature is measured on the tail is it not?

Davis Yes *at this area of the tail*

Hart Do you know about the skin temperatures in other areas of the animal?

Davis Yes we measured the paw temperature

Hart *Other skin temperatures for example under the fur?*

Davis Under the fur? Since it didn t produce much change even in the shaved animals we didn t worry too much about it

I think your point is that the bare surfaces we were measuring *represent a relatively small proportion of the total* skin area of the rat

Horvath What sort of diathermy are you talking about? Did you say 10 cm or 10 meters?

Davis *I said 10 meters*

Horvath As far as I can tell from all the literature which has been published 10 meters is definitely a local effect first and a deep effect second

Davis In the cold?

Horvath Not in the cold

Davis This is in the cold

Horvath That is what worries me why I can t quite understand

Davis It can be shown in the mouse quite readily In fact I showed it first in the mouse I think there are several reasons why the skin temperature has remained low The literature on diathermy shows that it heats up hot areas more than cold areas (4 13) This is one reason

Another reason is that vasoconstriction is a biological phenomenon It is responding to air temperature not altered by diathermy This could be straight vasoconstrictive effect

Still another reason is that the amount of diathermy required to

alter rectal temperature may be insufficient to overcome the heat loss of the skin to the cold environment This is in the cold

Also we find that if about 10 cm are used which you mentioned a heating of skin rather than of deeper tissues will result

Horvath To understand this I would have to know what kind of heat you are using

Burton Are you using condenser plates?

Davis Yes

Burton Then it is electrostatic heating Are you talking about that or electromagnetic heating ?

Behnke How long could you have maintained the low skin temperature and the normal rectal temperature by means of diathermy? I am thinking of the important application of this procedure in treating burn patients You have done something which is rather remarkable in lowering oxygen consumption and maintaining deep body temperature despite the low peripheral temperature How long can you maintain it?

Davis I think it can be maintained for a long time

Behnke How long have you maintained it?

Davis I maintained the obese hyperglycemic mice which died in a short time in the cold for as long as 4 days

Behnke And the skin temperature was ambient temperature?

Davis Yes skin temperature was down and there was still pilo erection

Travell Did they shiver?

Davis I couldn t see that I didn t measure the shivering in obese hyperglycemic mice I also maintained nonobese mice this way Shivering can be measured until that point where vasodilation takes place if the diathermy which interferes with the EMG is stopped

Burton I made some rapid rough calculations From the rise of temperature in 10 minutes you certainly are not putting more than 4 watts of energy into the mouse

Davis I would imagine it is a very small amount Using this technique we felt that we could more adequately investigate the relationship of shivering to the thermal state of the core and the state of the periphery as measured by skin temperature (8) Admittedly this is only approaching an old research problem using a different technique Figure 76 shows the presence or absence of shivering with various combinations of core and skin temperatures These are results obtained from mice Figure 76 is divided into five sections in accordance with the thermal state of the core The solid line in the

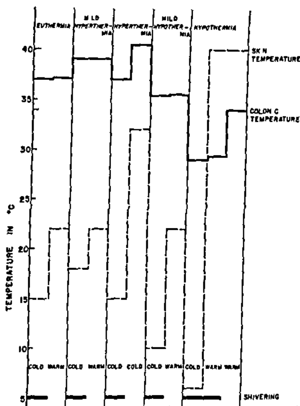


FIGURE 76 The influence of central temperature and skin temperature on shivering

upper part represents colonic temperature, the interrupted line represents skin temperature and the solid bars at the bottom indicate the presence of shivering

In the first section, shown on the left hand side of Figure 76, the animals were studied in euthermia They were placed in the cold until visible shivering occurred, at which time rectal and skin temperatures were recorded In this case, shivering became apparent at a mean skin temperature of 15°C, and a core temperature of 37°C Skin temperature was then rapidly raised with a radiant heat lamp until shivering disappeared, and again rectal and skin temperatures were recorded As can be seen, shivering was abolished at a skin temperature of 22°C with a very small change in core temperature In the next section of Figure 76 a mild hyperthermia was induced with diathermy and the animals were placed in the cold until shivering occurred, again, core and skin temperatures were recorded In this case, shivering occurred at a core temperature of 39°C with a

skin temperature of 17.5°C. Skin temperature was again raised rapidly with a radiant heat lamp. Shivering was abolished at a skin temperature of 22°C, and the core temperature was seen to remain at 39°C. In the third section, euthermic animals were put in the cold until shivering was established with core and skin temperature readings of 37° and 15°C, respectively. The animals, while still in the cold, were then subjected to a high intensity of diathermy until shivering ceased. This occurred at core and skin temperatures of 41° and 32°C, respectively. Thus, up to this point in Figure 76, it would appear that shivering in the euthermic and the mildly hypertherm c

lation inhibiting shivering (8, 10).

Irving: Are these also measurements of the tail temperature?

Davis: Yes.

Carlson: What is the period?

Davis: What determined the period was the time of exposure in the cold required to produce visible shivering and the time of warming required to inhibit shivering.

Carlson: So the skin temperature is given.

Davis: Yes. In the middle section we find, again, that, once we had raised central temperature above 40°C in the cold, we produced a vasodilation with cessation of shivering.

Since the obese hyperglycemic mice exhibit shivering and are easily made hypothermic, they were chosen for the experiments de

f shivering

the mice

duced and

r skin tem

perature. Skin temperature again was rapidly raised with a radiant heat lamp until shivering was abolished. This occurred at core and skin temperatures of 35° and 22°C, respectively. In the last section, a deep hypothermia was induced in these animals where core temperatures were allowed to fall to 28°C and again the accompanying shivering was in evidence. Skin temperatures were then rapidly raised with a radiant heat lamp to a temperature of 40°C. The accompanying rise in core temperatures was shown to be approximately a half degree. In spite of the high skin temperature, shivering was not abolished until colonic temperature had reached approximately 34°C (8).

The solid line shows the colonic temperature when we stopped

producing hypothermia Shivering was quite visible at this temperature We raised the skin temperature with the radiant heat lamp to as high as 40°C We kept heating because we found we couldn t stop shivering Shivering did not stop until colonic temperature was brought up to around 34°C

It was concluded therefore that in mild hypothermia shivering was still dependent upon skin temperature stimulation In deep hypothermia shivering appeared to be dependent upon the low core temperature and shivering could not be abolished by raising skin temperature alone until core temperature had risen to a less severe hypothermic state (8)

We may conclude further that there is a range of the thermal state of the core where shivering is primarily stimulated by changes in skin temperature Above a certain degree of hyperthermia in this case 40°C shivering cannot be produced because heat loss requirements are paramount I think the hypothalamus or wherever this shivering center is takes over and inhibits shivering by producing vasodilation In the hypothermic end of the range there is a point below which no matter how much the skin temperature is raised the central temperature continues to stimulate shivering

Lyman You mean hypothermia?

Davis Hypothermia I want to point out what I tried to term the physiological range whereby shivering appears to be stimulated mainly by changes in the skin temperature and how there is a range of hypothermia beyond which shivering appears to be a centrally stimulated mechanism

Burton I have said this many times This isn t the only interpretation You may say the view I express is really the same thing but I think it is more sophisticated in a physiological sense

I prefer to believe that the hypothalamic center is utterly dependent on the afferent stimulation from the skin but what it does with that afferent stimulation depends upon its own temperature Perhaps it is too crude to say that in one range the temperature of the hypothalamus is regulating shivering It is always regulating It is like the switchboard operator at the telephone exchange Her efficiency depends upon how warm or cold she is

This I think is a much more sophisticated view than the other which might lead us to look for a thermal receptor in the hypothalamus and I don t think such a structure exists

Keller This view also entails considerable supposition does it not?

Irving I don t think Dr Davis said anything about the central

nervous system He is wise to keep away from it because he hasn t investigated it

Keller Another explanation would be two types of receptors one activated at a much lower level than the other Both types of receptors could be in the skin as far as that is concerned

Carlson Lim s (14) experiments seem to rule this out He perfused the head separately and showed the temperature of the head was a wide range control and depended on the input as Dr Burton has said

Burton He was working with panting the other end point I always regretted he didn t do the same beautiful experiments with shivering as well

Keller His experiments were not uncomplicated there remained more than one variable

Carlson Lim* has done some experiments with cold and is at present extending this work

Davis A more sophisticated use of diathermy hinges upon its apparent ability in the cold to raise core temperature or body heat content without affecting skin temperature In doing this the cold induced oxygen consumption is reproducibly depressed We therefore considered that we had something which perhaps would allow us to examine the dual control of metabolism that Richet (15) proposed in 1898 whereby he thought that there was a part of metabolism which was primarily controlled by changes in central temperature and another part which was primarily controlled by changes in core temperature We feel that diathermy used at the correct levels of intensity does this for us I think Hemingway (16) did some work on this also Figure 77 shows how curare and diathermy fractionate metabolism in the cold in the probable manner that Richet proposed The ordinate is oxygen consumption in milliliters per hundred grams of rat Along the bottom of the bargraphs are the conditions under which oxygen consumption was obtained The first bargraph is basal oxygen consumption obtained in a warm environment The second bargraph represents the total oxygen consumption of untreated animals in a cold environment

In the next bargraph the animals were treated with diathermy at an intensity of 50 watts for a period of 10 to 15 minutes a sufficient amount of time to allow us to measure oxygen consumption The

*Lim T P K Elucidation of the role played by central and peripheral temperatures at the ation of shivering *Lovelace Foundation Report* N Albuquerque N M July 21 19 8

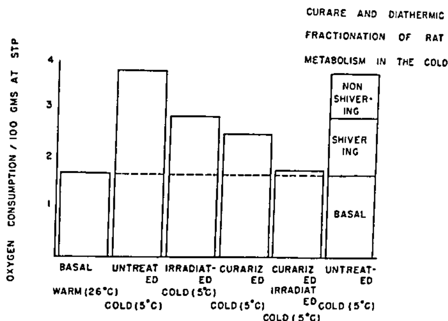


FIGURE 77 Curare and diathermic fractionation of rat metabolism in the cold

characteristic depression of oxygen consumption occurred in all of the animals In the fourth bargraph, the animals were curarized and a depression of oxygen consumption occurred which was equivalent to that amount of the cold induced oxygen consumption in the previous bargraph, which could not be removed by diathermy This indicates that the part of cold induced metabolism which could not be removed by diathermy represents the metabolic contribution of shivering (17)

The amount that the oxygen consumption could be depressed by curare happens to be equal to the amount of oxygen consumption which remained during diathermy These experiments were on twelve animals and were very reproducible

If this deduction were true then a combination of curare and diathermy should remove all the cold induced metabolism The fifth bargraph shows the results of animals treated with curare and diathermy, and practically all of the cold induced metabolism has been eliminated We therefore concluded that heat production in the cold of unacclimatized animals consisted of three fractions a basal fraction, upon which is added a shivering fraction, which amounts to approximately 55 per cent of the total cold induced metabolism, and

a nonshivering fraction contributing about 45 per cent of the total cold induced metabolism in the rat (17) Our findings in mice with the same technique showed a 50 50 contribution of shivering and nonshivering heat production (18)

Bass I can t speak with any authority at all on curare but some colleagues of mine who have tried to curarize rats have reported that it is very difficult almost impossible

Davis Our criteria for curarization were as follows first that the animal be curarized for a sufficient period to measure oxygen con sumption second that curare should not produce death after the experiment and third that the animal collapsed as a result of curare administration (10) The dosages must be adjusted to the third and fourth decimal place per hundred grams of rat This is reproducible

Lyman How do you administer curare?

Davis Intraperitoneally

Travell What dose?

Davis In this case 0 0157 mg per 100 gm of rat Dr Lyman gave me some curare which he claims Walter Cannon used He claims that Cannon must have gotten it from scraping South American arrowheads

Keller How often did you inject into the lumen of the bowel?

Davis I don t think this occurred

Keller It never occurs?

Davis I have never done it in humans nor have I done it in animals The bowels move out of the way without difficulty in normal subjects

Keller Yes if you open the abdomen and watch the bowels But when the abdomen is closed in the rabbit cat and dog care must be taken not to puncture the intestine with the needle

Behnke Was the deep temperature maintained normal in all ex periments?

Davis Yes in all experiments The other criterion was that we maintain temperature which we could measure before and after the curarization and also measure oxygen consumption before and after to see that heat production had returned to normal after the effect of curare had ceased (10)

Behnke In all experiments the animals recovered?

Davis Yes

Irving Did you have to administer artificial respiration in your stage of curarization?

Davis No We found that if we curarized the animal very fully

and applied artificial respiration, in less than 5 minutes there was a fall in colonic temperature of up to 2 degrees This, of course, ruined our experiment This is why we retained these criteria, even though we had to be very careful to have the technique well under control

Burton What evidence is there that inclines you to feel that diathermy, by raising the central temperature, the brain temperature, inhibits only the nonshivering part? Why shouldn't it be inhibiting both?

Davis Because curare removed the part of oxygen consumption which was not depressed by diathermy

Burton Couldn't the diathermy have been removing part of the shivering and part of the *hypothetical nonshivering?*

Davis It could have

Burton What inclines you to say that the part removed by diathermy was nonshivering'?

Davis Because you could not depress the oxygen consumption further by diathermy except by raising the skin temperature or administering curare, which eliminated shivering

Burch Were electromyograms obtained?

Davis Yes Is your question answered, Dr Burton?

Burton I will have to think about it There has not been sufficient time yet for me to know whether or not I find this answer acceptable

Carlson If he had the EMG that is the answer

Burton Yes, if he had evidence that this diathermy was not re*moving the EMG but was removing the heat*

Davis This is not complete evidence because of the variability of shivering from one minute to the next and from one animal to another So you still are faced with the original problem

Burch The shivering phenomenon would be different

Davis The shivering phenomenon as I will show later, is a highly variable factor

Horvath Isn t it your electromotive energy you are measuring? You either did or didn't have it

Davis There was the strychnine effect of curare at the *beginning* when curare started to take effect It was hard to distinguish between this and muscular activity, but later, during the period of curare effect, almost all EMG was wiped out

The results just presented were obtained from unacclimated animals It was of interest to determine what happened to these two fractions in the course of acclimation Figure 78 depicts the effect of acclimation upon oxygen consumption and shivering The ordinate represents per cent change of shivering and cold induced

metabolism or EMR The abscissa is exposure time in days Figure 78 depicts results on three groups of animals at three different environmental temperatures namely 13° 6° and 2°C The three upper curves are the changes in oxygen consumption The two lower curves represent changes in shivering activity for the group exposed to 13° and 6°C (10)

In 1953 Dr Lyman helped me considerably in obtaining the 2° and the 13°C curves Facilities failed me for the 2°C curve and I

But the important point here of continued exposure with luction

Thus we were seeing what Sellers (19) (who was doing his work at about the same time) has already pointed out that heat production can occur in the absence of shivering in cold acclimated rats

Figure 79 shows the oxygen consumption curves expressed as per cent of EMR of the same three groups of animals when irradiated with diathermy In all three groups we find that the oxygen consumption can be divided into two phases an early phase in which the oxygen consumption curves show a gradual fall and a later phase in which the oxygen consumption curves show a gradual reestablishment of initial levels Somehow the early phase appears to be associated with the disappearance of shivering but the correlation is at best poor In the second phase oxygen consumption curves show an increase at a time when shivering activity levels are nearing zero Since we had previously concluded that the heat production remaining during diathermy irradiation is caused by the continued stimulation of the low skin temperature we further concluded that the maintenance of these levels of oxygen consumption under dia

we should be able to measure the development and contribution of this fraction by a combination of curare and diathermic treatment (10)

Figure 80 depicts our results with curare treated animals as well as with curare plus diathermy treated animals The curves are oxygen consumption expressed again as per cent of EMR The upper curve shows oxygen consumption of animals treated with curare alone The lower curve shows oxygen consumption of animals treated with both curare and diathermy As acclimation proceeds the effect of curare in depressing oxygen consumption becomes less

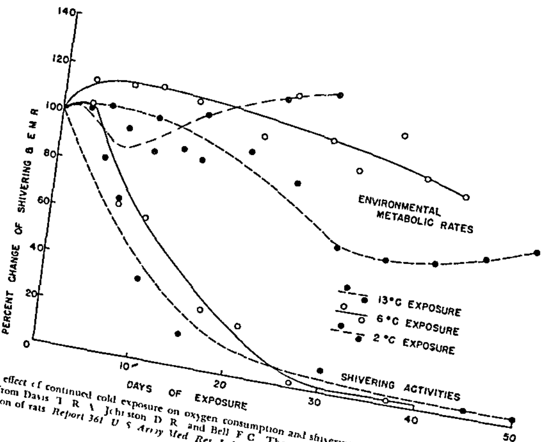
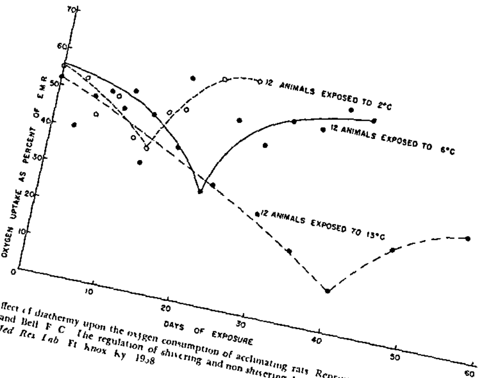


Figure 78 The effect of continued cold exposure on oxygen consumption and shivering at three different air temperatures Reprinted by permission from Davis T R A, Johnston D R and Bell F C The regulation of shivering and non shivering heat production during acclimation of rats *Report 361 U S Army Med Res Lab* Ft Knox Ky 1958



…URE 79 The effect of diathermy upon the oxygen consumption of acclimating rats Reprinted by permission from Davis T R A, …hnston D R and Bell F C The regulation of shivering and non shivering heat production during acclimation of rats *Report* … U S *Army Med Res Lab* Ft Knox Ky 1958

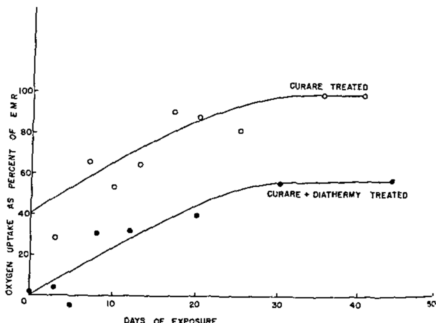


FIGURE 80 The effect of curare on nondiathermized and diathermized rats during acclimation Reprinted by permission from Davis T R A Johnston D R and Bell F C The regulation of shivering and non shivering heat production during acclimation of rats *Report 361 U S Army Med Res Lab* Ft Knox Ky 1958

and less (10 20) After 30 days curare has no effect upon total metabolism in the cold even though the criteria for curare dosage are satisfied Similarly as shown in the lower curve the effect of curare plus diathermy treatment in depressing the cold induced metabolism also becomes less and less and after 30 days of exposure curare has no effect in further depressing the oxygen consumption of diathermy irradiated animals Thus we can conclude that the lower curve in Figure 80 is in effect measuring the metabolic contribution of a skin stimulated nonshivering thermogenesis The upper curve can be interpreted as measuring (by subtracting from the total metabolic rate) the contribution of shivering heat production The correlation of these two curves with the disappearance of shivering activity is statistically significant (10)

Figure 81 represents a simple interpretation of what is occurring to shivering and nonshivering thermogenesis during acclimation The top curve represents the per cent of cold induced metabolism at any point during the period of acclimation The second curve represents the per cent contribution of shivering heat production to

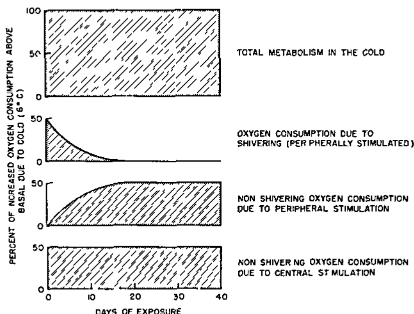


FIGURE 81 Fractions of metabolism which contribute to the cold induced metabolism during cold acclimation of the rat

cold induced metabolism as a function of exposure time The third curve is the per cent contribution of a skin stimulated nonshivering heat production The bottom curve represents the contribution of a centrally stimulated nonshivering heat production Thus cold induced metabolism at any time during the period of acclimation is the algebraic sum of shivering of a skin stimulated nonshivering heat production and of a centrally stimulated nonshivering heat production (10)

In these experiments we used something like 150 or 200 animals over a period of 4 years In some animals after acclimation no depression of total oxygen consumption would occur when treated with diathermy During these experiments we gained the distinct clinical impression that the animals were least resistant to cold between 10 to 15 days of exposure which was the period when most of the deaths occurred

The obvious thing to do next is to show the cold sensitive response as a function of exposure time We haven t done this Colonel Blair has already shown that the acclimated rat is resistant to cold

Reynolds Is it shivering you refer to in that increase in electromyograph or visible shivering?

Davis Increase in myograph The question that hasn t been asked I shall answer anyway It is where is all this nonshivering heat production coming from? Obviously all we have done is index its possible existence

Dr Keller as others have done implicated the liver as a source of heat production I felt that muscle may produce—what was your term Dr Burton?

Burton Noncontractile

Davis Noncontractile heat production Figure 82 shows the oxygen uptake of liver and muscle slices obtained by the Warburg technique (21) Each point represents ten animals and five to ten samples per animal The Warburg technique is a most unhappy one because of the great variability encountered The solid curve represents the oxygen uptake of liver slices The interrupted curve represents our findings on muscle tissue For liver tissue no significance could be obtained between zero and 30 days The 15 day point however is significantly low This low figure shows the opposite of what one would expect if liver increases its capacity to produce heat

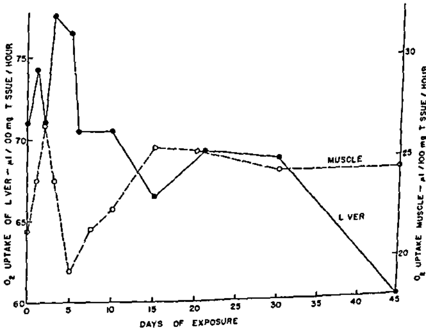


FIGURE 82 Oxygen uptake in muscle and liver slices of acclimatizing rats

In the muscle oxygen uptake goes up on the third day where there is an increase in shivering and an increase in total heat production in the cold. Then it falls at about the time that shivering shows a rapid fall in activity. When shivering has disappeared at about 20 to 25 days the oxygen uptake is significantly higher than during the nonacclimated state. I don't know what inference we can make from this except perhaps muscle does produce a noncontractile or isometric heat production in acclimated animals.

Hart Are the units of the ordinate expressed as per cent of initial values?

Davis No. O uptake of liver or muscle.

Carlson What muscle did you use?

Davis We used wet tissue from the quadratus.

Taylor Does the weight of the animal change the state of the tissue?

Davis After the biochemist pointed this out we tested for volume of water change. There wasn't any.

Burton It would be important in view of this suggestion about noncontractile thermogenesis. We can say these are nonelectromyographic activities of muscle. It would be important also very difficult to devise some way of actually cutting a tendon and measuring the tension exerted by a muscle wouldn't it to see whether it really was a noncontractile activity of muscle. It is nonelectromyographic.

Davis and I am just assuming it must be noncontractile.

Hart Is there any situation where there is contraction without an electromyogram?

Burton As far as we know this might be a new situation. It is important to know whether this is noncontractile whether or not the muscle is exerting tension.

Keller The muscle would require a double innervation.

Burton This would be new physiology but this is what we are looking for isn't it?

Keller The second innervation would need to be independent of the final common path used for shivering.

Carlson It could be humoral it doesn't have to be neural.

Keller That is quite correct.

Davis I think it has to be humoral unless we state two nerve pathways. This oxygen consumption if it does occur in muscle is occurring even though curare is administered.

Carlson Actually this change in muscle certainly occurs and is measured at 37°C and thus the rat would be expected always t

have elevated metabolism, whether it is warm or cold The significant point is that the cold stimulus *is what is giving the rise in metabolism*

A second point concerns the work of Arnold Hsieh in our laboratory (22, 23) We demonstrated that the curarized rat's oxygen consumption increases with cold exposure Sympathetic blocking agents and adrenolytic substances prevent this response, while it can be simulated by epinephrine and norepinephrine It seems to me that this would indicate a response to cold that would not be expected to measure in the Warburg unless the conditions could be duplicated

Davis I didn t expect to see it happen the way the results show and as I pointed out, all we have shown is the relative ability of *muscle to take up oxygen in vitro*

Hart I fully agree with Dr Carlson that we need *in vivo* experiments We should measure the metabolism of different organs simultaneously with measurement of whole animal oxygen consumption Somehow, we must determine the oxygen consumption of the organs by measuring the atrioventricular difference and blood flow

Davis The method is complicated by flow changes

Hart We have used the elimination technique Depocas (24) has used the method of curarization plus functional evisceration Even under these circumstances, it is possible to obtain a cold induced rise in oxygen consumption in rats exposed to cold, after they were previously *cold acclimated We do not conclude that this necessarily* implicates skeletal muscle but it virtually eliminates an important part of the viscera, including the liver, as a source of heat production

Behnke Why not take histological sections of muscle and *count the* open capillaries?

Davis Hasn t O Heroux of the Defence Research Laboratories in Ottawa done this?

Hart Yes There is increased vascularity Actually, his most certain results concern peripheral tissues, the ears of rats The work on muscles has been re examined

Keller Interpretation of vascularity of an organ is as difficult as interpretation of the magnitude of glycogen in the liver, histologically, because the end result depends so much on several hard to quantitate factors

The variability there may be considerable It is difficult to draw conclusions, except in a rough, semiquantitative manner

Davis If Figure 83 looks confusing, it is meant to It shows the shivering patterns and *their times of onset of* nine human subjects

exposed in the nude to a temperature of 14°C for a period of 60 to 70 minutes The ordinate is electrical activity of shivering expressed in volts per minute obtained by integrating electromyographic recordings of muscle action potentials obtained from the thigh All these recordings were obtained in the month of October and amply demonstrate the variability of shivering from individual to individual The times of onset vary from 4 minutes to 56 minutes of exposure The maximum activity obtained during the period of exposure varied from 19 to 100 volts

Siple Can you tell us the conditions?

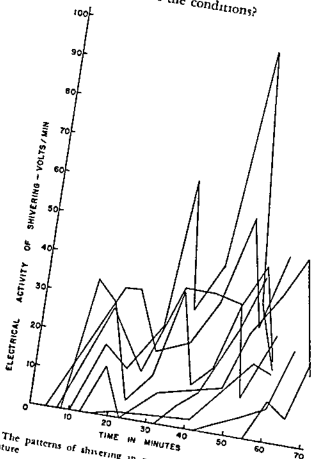


FIGURE 83 The patterns of shivering in nine human test subjects exposed to 14°C air temperature

Davis The conditions were 14°C exposure in the nude, lying down for one hour Actually, we took some of these for more than one hour, but I am giving you the hour data

The characteristic cycling of shivering is also demonstrated where peaks of activity occur with periods of quiescence These appear to be fairly constant for each individual but vary in periodicity from 15 minutes to 45 minutes between peaks Periods of bursts of activity are characterized by an increase in frequency and amplitude of the short bursts associated with shivering The periods of quiescence are characterized by a tonic type of contraction At first glance, it would appear that this mechanism of heat production is too crude to be relied upon to maintain the smoothness associated with homeothermy With the instrumentation available, it was not possible to relate these periods of activity and quiescence with changes in colonic or skin temperature

We carried this on in some individuals for 2 hours, to see whether this had a cyclic trend, it does appear to have one For instance, one of these individuals is I, 1 peak every 22 minutes, plus or minus 1 or 2 minutes, for the whole 2 hours

Henschel Does the pattern change if the environmental conditions are changed?

Davis If the environmental conditions are changed, the pattern does change It also depends upon the time of the year

Burton It depends also whether it is after lunch or before lunch

Davis Yes, it appears to If shivering is re-established in winter by lowering the temperature of the room, it is often possible to eliminate these peaks But when shivering is occurring at the highest period of activity, you usually get these bouts of shivering separated by periods of relative quiescence

Fremont-Smith Shouldn't degree of nervous tension be considered, also?

Davis We are trying to eliminate this

Fremont Smith If it does occur, it will operate to accentuate the bouts of shivering?

Davis Yes We could inhibit shivering for as long a time as 5 minutes and as short a time as 30 seconds, but we could not change the over all pattern

Figure 84 shows the effect of immersion in sea water at 11°C on a human subject, insofar as his rectal temperature and shivering response are concerned The rectal temperature curve is based on actual measurements The skin temperature curve below is esti

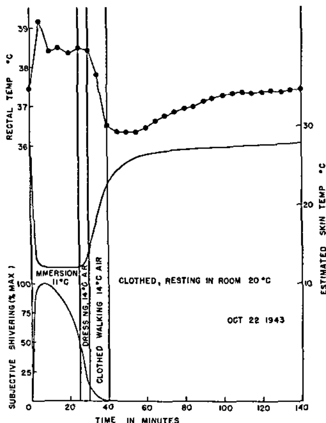


FIGURE 84 Rectal temperature changes in man during and after immersion to 11°C sea water

mated and the shivering curve is based upon subjective observation The abscissa is time in minutes (8) My lack of concrete data is caused by the fact that my laboratory was a beach my instrumentation was an ordinary clinical thermometer and I had nothing else except my own biased powers of observation

The period of immersion occupies the first 25 minutes followed by the postimmersion period consisting of a dressing period a walking period and a resting period This experiment was repeated six times at somewhat different prevailing sea and air temperatures but the results were essentially as shown here During immersion the rectal temperature showed a marked rise at the end of 5 minutes

followed by a maintained level at temperatures significantly higher than preimmersion temperatures During this period of hyperthermia there was violent shivering The estimated skin temperature was low during this period Following immersion the rectal temperature fell precipitously to below the pre experimental level and it did not return to this level until about 2 hours after immersion During this period of postimmersion hypothermia no shivering could be demonstrated objectively or subjectively and the estimated skin temperature had returned to near the pre exposure level Therefore we were led to conclude that under the conditions of the experiments shivering is primarily stimulated by the effect of cold upon the skin and that the immersion hyperthermia as well as the postimmersion hypothermia had little influence in inhibiting or stimulating shivering

Since this experiment we have found that the human unlike the rat ceases typical bursts of shivering as soon as he is taken out of the cold The rat on the other hand continues to exhibit typical bursts of shivering for an appreciable time after removal to a hot room We feel that this difference is dependent upon differences in insulation We have not observed postexposure shivering in a clothed man to test the validity of this premise Although the typical bursts of shivering activity disappear in the nude man on removal to a warm room continuous tonic contractions which are easily inhibited by psychic stimulation can be seen on the EMG

Siple This isn t always true in extreme cold at least Under such conditions a man comes in after he has been outdoors working for hours at a time without shivering—

Davis Remember he is insulated by clothing

Siple He is adequately dressed but shortly after he comes indoors where it is warm he begins to shiver

Keller In that situation he can t shiver while working because his muscles are in use

Davis This is the best inhibitor of shivering

Siple He didn t feel cold when he was outside but when he got inside he felt suddenly very cold Purely as a layman I would say that once he hit the warm air actually there was a certain tendency for vasodilation to commence Circulation of the blood through the cold surface areas tends to bring about a rapid if minor drop in deep body temperature There is a whole series of psychological factors that go right along with these too

Blair In other words the clothing is still just as cold as outside and he circulates his blood through the cold peripheral tissue

Siple Vasomotor controls start to let warm blood flow through cold tissue because his body senses it is safe now that he is in a new warm environment

Davis We did this some years ago What this merely points out is the fact that with the skin temperature low there is hypothermia accompanied by maximum shivering During the postimmersion period we find a high skin temperature accompanied by hypothermia and no observable shivering

Figure 85 shows the effect of seasonal change on heat production and shivering activity Each of six individuals was subjected to a 1 hour exposure of 14°C temperature once during each month Shivering as indicated in the left hand curve and expressed as per cent change of initial fell to near zero levels during the months of December January and February Heat production expressed as per cent above basal decreased from a November value of 56 per cent above basal to a February value of 28 per cent above basal Un

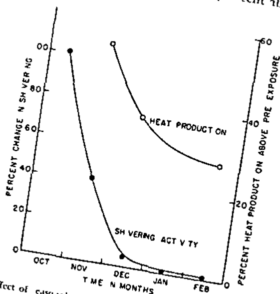


FIGURE 85 The effect of seasonal change upon metabolism and shivering of six human subjects

fortunately, we obtained October levels of heat production using the normal clinical methods of measuring metabolic rates consisting of pure oxygen breathing, and a group level of 60 per cent above basal was obtained We have not included this figure because, according to Dr Burton, breathing pure oxygen inhibits shivering We looked for this possibility but were not able to substantiate it under the conditions of our experiment which were such that breathing pure oxygen occurred only intermittently The value of 28 per cent metabolism above basal is significantly higher than levels obtained under basal conditions Thus, we feel that we have shown the presence of nonshivering heat production occurring in man Such changes in heat production and shivering activity indicate that since these individuals received only 1 hour of deliberate cold exposure per month, these changes could only have occurred as a result of seasonal change (25)

Hart What is the time course of an individual experimental procedure? In other words, there might be a lag in the response of heat production to the cold exposure How long did this test last and what were the changes?

Davis These are heat productions at the end of one hour The shivering levels are the mean for the whole hour

Bass What were the subjects doing 1 or 2 hours prior to exposure?

Davis They were brought in the night before and put in as basal a state as was possible They slept in They were put in a warm room at 30°C, to stabilize there for 1 hour and then into the cold for 1 hour Those were our experimental conditions

Figure 86 shows the October means and the February means of skin and rectal temperature The closed circles are the October and the open circles are the February figures for these individuals We found a change in rectal temperature between October and February It is not significant statistically, but it may mean something physiologically (25)

Skin temperature, on the other hand shows no change between October and February The mean onset of shivering for October occurred Since mean skin temperature did not change between October and February. ... was greater than 60 minutes It appears, therefore, that a shift of the skin temperature threshold required to stimulate shivering has occurred Since mean skin temperature did not change between October and February the heat loss from the skin surface did not change Yet we are faced with an explanation for the change of 50 per cent for heat production between October and February

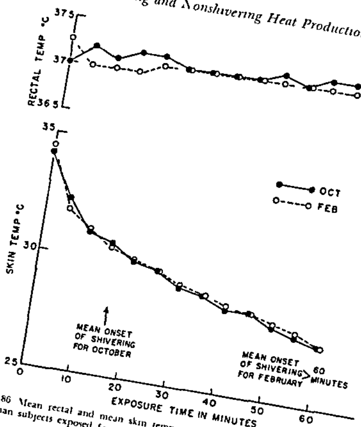


FIGURE 86 Mean rectal and mean skin temperature during October and February of six human subjects exposed for one hour at 14°C air temperature

Burch Where was the skin temperature measured?

Davis It was measured on the face, the hand and the forehead

Burch Did you study the palm of the hand?

Davis We studied the dorsum of the hand and the upper arm. We didn't measure the lower arm as we didn't have enough thermocouples. We studied the trunk, the chest, the abdomen, the upper thigh, the lower leg and the foot, and we also measured one toe

Fremont-Smith What is the variation in different parts of the body? You got a mean here which, as I understand it, is of all those

Davis We did analyze that. We found no significant change from each of the areas

Fremont-Smith You found the same curve on each area?

Davis Yes

Molnar There was a change in one subject I looked at very carefully

Davis There was a change He did go down a little and come back up from month to month We couldn t tell what was happening It wasn t associated with anything else

Molnar I thought we associated it with the skin surface temperature of the thigh

Davis We thought we had discovered something but, when we looked at it more critically and tried to apply it to a variable, we found it was contradictory

Molnar When I was looking at it, it was not contradictory, but I am talking from very poor memory, as it was some time ago

Davis This was the individual shown in Figure 87 I am using him as an example of what happens But, if I remember correctly, Dr Molnar, we looked into this, and we are certainly going to look into it more We have all the data plotted out for you

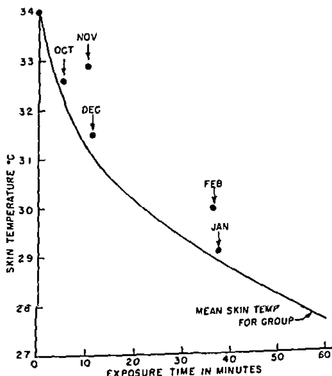


FIGURE 87 Mean skin temperature and time of onset of shivering in subject as a function of seasonal change

Burton When you say that the apparent difference is not statistically significant did you use the really appropriate kind of statistics which is paired statistics? Did you work on the means or compare the response in February and October on the same subject?

Davis We worked on the means as well as the differences

Burton I would have guessed that would come out significantly on paired statistics if those were the means

Davis I think we got a difference of 5 per cent which I am not willing to think is significant I do agree something definitely is happening here In our next study I think you will see what the difference is

Burton You don t accept 5 per cent as a workable level of confidence?

Davis I think we tried paired sample technique but we were not impressed

Figure 87 shows the onset times of shivering and mean skin temperature at which shivering occurred in the individual who shivered throughout the period of observation The ordinate gives skin temperature the abscissa gives exposure time and the solid line shows the mean skin temperature for the group In October this subject shivered after only 4 minutes of exposure at a relatively high mean skin temperature As fall and winter progressed the onset time of shivering increased with a lower mean skin temperature at which shivering occurred Since the subjects were exposed deliberately to cold for only one hour each month for testing purposes only these changes must be due to seasonal acclimation On the basis of these results we have presumed that the disappearance of shivering during acclimation is accompanied by a shift in the skin temperature threshold at which shivering is stimulated (25)

Figure 88 shows the effect of heat acclimation upon the shivering levels of the seasonally acclimated individuals used in the previous study We did use additional subjects and divided them into an exercise and a nonexercise group We found that even though we could satisfy the criteria for heat acclimation we could not significantly show a change between the pre heat and post heat levels of shivering Such a result would indicate that perhaps heat and cold acclimation can occur in an individual simultaneously and independently

Since it appears that shivering activity diminishes as a result of seasonal cold exposure it was of interest to determine the rapidity with which such change could be effected by an exposure under controlled conditions Therefore ten nude individuals were exposed

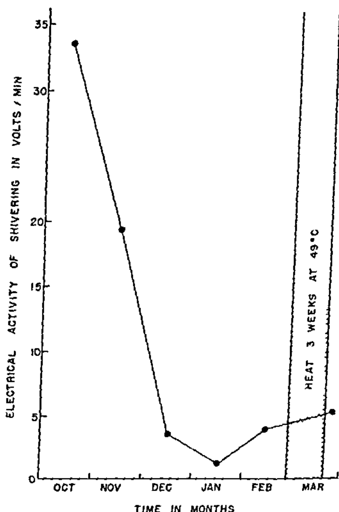


FIGURE 88 The effect of heat acclimation on seasonally cold acclimated human subjects

in a cold room at an air temperature of 12°C for 7 hours a day for a total of 32 days, 1 day a week being allowed for religious attendance Shivering, oxygen consumption, and rectal and skin temperatures were measured on each individual after 4 hours of the 7 hour daily exposure period

Figure 89 shows the changes in shivering and oxygen consumption that occurred during the 32 days of exposure The upper curve is heat production and the lower curve is shivering activity Shivering activity can be seen to fall rapidly in the first 10 days of exposure

reaching a low point at about 21 days This particular study was carried out during the winter—in March to be precise We found that we could re establish the shivering levels to only 50 per cent of the October levels even though the cold room temperature was lowered by 2 degrees and the exposure time was increased from 1 to 4 hours for measurement purposes The heat production curve can be seen to rise on the third day followed by a fall to approximately 33 per cent above basal This level was significantly above basal at the 1 per cent level of confidence indicating the presence of a nonshivering heat production since shivering is at low levels after the 20th day (26)

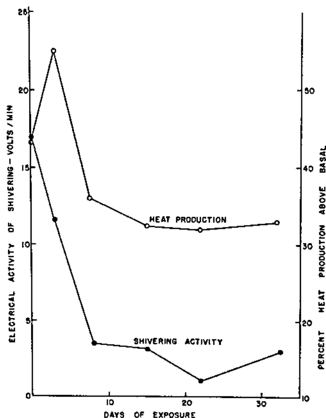


FIGURE 89 Oxygen consumption and shivering activity of ten human subjects exposed to 12.5°C air temperature for 7 hours daily for 32 days

Burch Was the heat production at 49 per cent in March?

Davis Yes, but these were basal and were taken at room temperature

Burch Did you check the heat production in those individuals?

Davis These were checked every day Figure 90 shows this change of rectal temperature you were looking for, Dr Burton At 15 days the rectal temperature has fallen quite significantly from the control levels (20, 21)

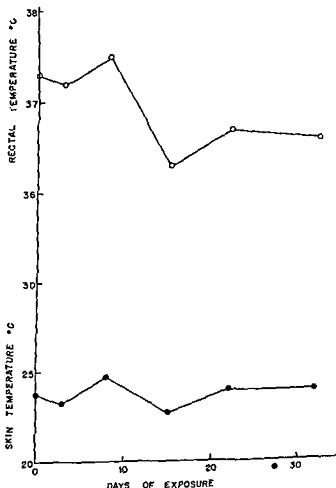


FIGURE 90 Mean rectal and mean skin temperature of ten human subjects exposed to 12.5°C air temperature for 7 hours daily for 32 days

Burton Is this the rectal temperature after 1 hour exposure?

Davis In this situation I should explain again that we did not measure shivering until the person had been in the cold room exposed for 4 hours and had been lying down for three quarters of an hour before we made any measurements

Burton Yes but does this rectal temperature represent what it has reached after the cold exposure or what he was before?

Davis This is while he was being exposed after 4 hours

Burton It might have started at a normal level and dropped during the exposure?

Davis No we did this all at the same time of the day throughout the period

Burton I would be more interested in the measure of drop during exposure I wondered if these data represented that or not

Davis No The first record is the first day of cold exposure and the next is 3 days the next 8 15 22 31 and 32 days

We notice again that the skin temperature had not changed Actually the changes that did occur were because of the vagaries of our cold room but the difference between the skin temperatures and the room environmental temperature was the same throughout the experiment Again we see that the heat loss at least from the skin surface is the same throughout the period of exposure Having seen the seasonal response we have to specify what time of year we did this this is the winter study We intend to study it again in summer when the background of the Kentucky heat acclimation has taken place

Reynolds If there were seasonal effect you would expect it to happen in the spring?

Davis I am not sure When you keep people in the hot room at 49 C for 3 weeks and do not get a change in our parameter one wonders Essentially these experiments indicate that man appears to show physiological changes as a result of continued cold exposure These changes are essentially the same as those seen in the rat where an accompanying increase in resistance to local cold injury and hypothermia has been demonstrated One perhaps would not be too fanciful if one presumed that a similar increase in resistance to cold is also occurring in man as a result of continued exposure to cold especially when we consider the tremendous change in energy requirements between acclimated and nonacclimated individuals

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THE PROBLEM OF EQUIVALENCE OF SPECIFIC DYNAMIC ACTION EXERCISE THERMOGENESIS AND COLD THERMOGENESIS

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IN AN EARLIER PAPER (1) I considered the equivalence of specific dynamic action I therefore now will present only material on equivalence of exercise and cold thermogenesis We are all in concurrence with the principle that heat whatever be its origin or mode of production is potentially available for maintaining body temperature in a cold environment However for purposes of temperature regulation it is quite possible that heat produced as a by product of one type of activity may not be as effective as heat produced as the by product of another type of activity That is to say the activities may not be equivalent or equally efficient It is the problem of equivalence of various modes of heat production that I wish to discuss Equivalence here will be defined in terms of metabolic replacement Two sources of heat will be considered equivalent if one is able to substitute metabolically for another in a cold environment Under these conditions there would be a gain of efficiency in the sense that an activity helping to maintain body temperature would at the same time and without increased energy expenditure be performing some other function such as mechanical work

Figure 91 will illustrate the principle involved The upper panel of this figure illustrates the classical principle of equivalence where metabolism is related diagrammatically to environmental temperature The metabolism in a resting animal typically shows a thermal neutral range A – B with the critical temperature at point B and an increase in heat production B – C along the line roughly proportional to the temperature gradient between the central body temperature and the air

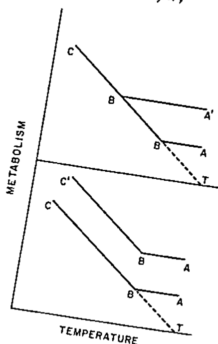


FIGURE 91 Demonstrates the effect of replacement of cold thermogenesis by an activity with lowering of the critical temperature from B to B′ (upper graph) and addition of heat production from an activity to the cold thermogenesis (lower graph) ABC is heat production during rest A′B C′ is heat production during the activity T is body temperature

If another activity, such as exercise, which increases heat production in the neutral range, A — B, is superimposed the principle o equivalence requires that the critical temperature is lowered to B′, because the activity substitutes for the cold thermogenesis i e, the one replaces the other

The lower portion of this figure shows the situation where there is no equivalence but addition of the cold thermogenesis to that from another activity That is, there is an increase in the heat production from the activity, not only in the neutral range A — B but also in the cold as well The heat production is higher than it would be if the additional activity had not taken place In other words, there is an increased energy cost in the cold

There is another important aspect of equivalence that will be considered and that concerns the origin of the heat producing processes If an activity such as exercise or specific dynamic action of food produces heat that substitutes for that which would normally be

produced in response to cold the substitution would implicate the same common pathway for the two activities On the other hand nonsubstitution seems to disfavor the utilization of a common pathway at least in the examples to be given

Carlson Isn t the graph also constructed on the assumption that the exercise doesn t change the heat loss in the animal?

Hart Yes as I will point out later that is one of the assumptions or one of the conditions that is perhaps being overlooked We are thinking here only in terms of metabolism Of course if heat loss is changed there will be quite a different picture I intend to go into this question of what happens to heat loss

Irving Would not also your lower example necessarily represent an elevation of the body temperature?

Hart Yes

Fremont Smith Are you saying that the body can be kept just as warm by heat produced in one organ as by heat produced in another and that the calories produced in one place will be an adequate substitute for calories produced somewhere else to keep the body warm? Or are you saying something more than that?

Hart I am saying this could happen

Fremont Smith That is essentially what you are saying could happen?

Hart Yes

Horvath It depends entirely upon the integrity of the circulation

Hart In other words on the insulation

Fremont Smith I was simplifying it provided heat can be transported appropriately

Hart What I would like to discuss mainly in this talk is the question of exercise and cold thermogenesis I have been working for a number of years on it If there is time later I will mention briefly the question of specific dynamic action but only to illustrate the principle further

During exercise the origin of the heat production is from muscular contraction Cold thermogenesis originates both from muscular contraction (shivering) and from noncontractile processes I will deal separately with the interaction of exercise with shivering and nonshivering thermogenesis I have been aided of course by the previous speaker who has defined what he means by shivering and nonshivering thermogenesis The meanings to be given to these terms are approximately equivalent to those of Dr Davis

At the outset classical theory has always favored the idea that

during exercise, the heat produced will substitute for that which would be normally produced in a cold environment with a resultant gain in efficiency

This idea goes back to the old views of Rubner (2), whom few people seriously question today It is generally accepted that if shivering is the source of heat in the cold, exercise would inhibit shivering because both processes originate from muscular contraction Hence one would logically think that the one would substitute for the other However, there is very little direct evidence for this

Figure 92 provides electromyographic evidence on the elimination of shivering by exercise When a pigeon was placed in a room at 20°C, we obtained the electromyograph shown on the upper left

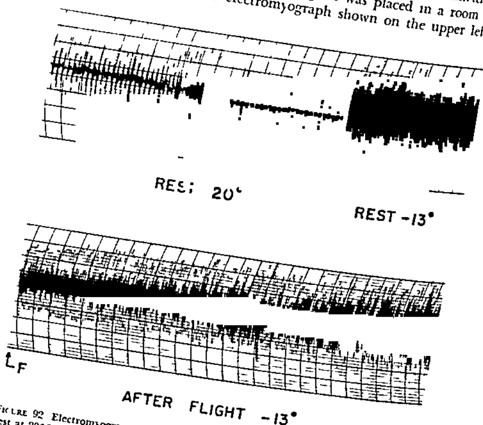


FIGURE 92 Electromyograms recorded from breast muscles of a pigeon with bird at rest at 20°C (upper left) at rest at −13°C. (upper right) and immediately after flight (F) at −13°C The ECG is superimposed on the EMG at 20°C. but is obliterated by shivering at −13°C After flight ECG can again be seen

hand side of this figure The heart activity and the muscular activity which is very low are evident

When the bird was placed at −13°C the shivering activity increased very quickly and completely obliterated the electrocardiogram The record was obtained after 1 hour at this temperature with the bird at rest and confined in a small cage

What happens when we make him fly? No records were obtained during flight because the electrical activity completely masks any other simultaneous activities The lower record shown in Figure 92 was taken immediately after flight At first nothing but the increased heart rate can be seen Gradually after a few seconds shivering reappears and again obliterates the pattern of the electromyogram

Siple How long was the flight?

Hart This flight was in a cold room and was terminated after the bird had flown about 10 feet

Horvath Don't you see this occurring spontaneously this intermittent type of shivering and nonshivering? Man does it

Hart Not in the pigeon It is continuous I might say that pigeons seem to produce the main portion of their cold thermogenesis by shivering John Steen (3) has shown this very nicely in some of his electromyographic studies We have not been able to demonstrate nonshivering thermogenesis in these birds because they fail to elevate metabolism in the cold when paralyzed by curare *

Horvath They have ability to change insulation

Keller Doesn't continuous or periodic shivering depend on the cooling load? It does in dogs With a moderate cooling load periodic shivering obtains, which frequently is associated with the inspiratory act *i e* it is facilitated by inspiration Later if the cooling load is sufficient shivering becomes continuous and is not associated with respiration

Horvath This is not necessarily true in man but dogs are rather peculiar in that respect

Burton Do you interpret the bottom record as showing significantly less shivering than the one on the upper right?

Hart My impression is that the electrical activity immediately after this flight is very low and gradually comes back after the animal has been resting for a few seconds The time of each vertical bar is one second

Burton So you do think that is less at least on the left hand side?

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* Hart J S [illegible]

during exercise, the heat produced will substitute for that which would be normally produced in a cold environment with a resultant gain in efficiency.

This idea goes back to the old views of Rubner (2) whom few people seriously question today. It is generally accepted that if shivering is the source of heat in the cold, exercise would inhibit shivering because both processes originate from muscular contraction. Hence one would logically think that the one would substitute for the other. However, there is very little direct evidence for this.

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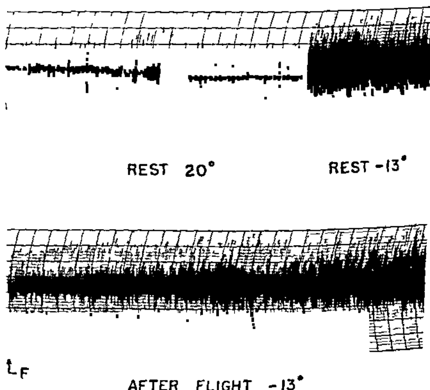


FIGURE 92 Electromyograms recorded from breast muscles of a pigeon with bird at rest at 20°C (upper left) at rest at −13°C (upper right) and immediately after flight (F) at −13°C. The ECG is superimposed on the EMG at 20°C but is obliterated by shivering at −13°C. After flight ECG can again be seen.

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What happens when we make him fly? No records were obtained during flight because the electrical activity completely masks any other simultaneous activities. The lower record shown in Figure 92 was taken immediately after flight. At first nothing but the increased heart rate can be seen. Gradually after a few seconds shivering reappears and again obliterates the pattern of the electromyogram.

Siple: How long was the flight?

Hart: This flight was in a cold room and was terminated after the bird had flown about 10 feet.

Horvath: Don't you see this occurring spontaneously, this intermittent type of shivering and nonshivering? Man does it.

Hart: Not in the pigeon. It is continuous. I might say that pigeons seem to produce the main portion of their cold thermogenesis by shivering. John Steen (3) has shown this very nicely in some of his electromyographic studies. We have not been able to demonstrate nonshivering thermogenesis in these birds because they fail to elevate metabolism in the cold when paralyzed by curare.*

Horvath: They have ability to change insulation.

Keller: Doesn't continuous or periodic shivering depend on the cooling load? It does in dogs. With a moderate cooling load, periodic shivering obtains which frequently is associated with the inspiratory act, i.e., it is facilitated by inspiration. Later, if the cooling load is sufficient, shivering becomes continuous and is not associated with respiration.

Horvath: This is not necessarily true in man, but dogs are rather peculiar in that respect.

Burton: Do you interpret the bottom record as showing significantly less shivering than the one on the upper right?

Hart: My impression is that the electrical activity immediately after this flight is very low and gradually comes back after the animal has been resting for a few seconds. The time of each vertical bar is one second.

Burton: So you do think that is less, at least on the left hand side?

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*Hart, J. S. Unpublished.

resting levels at certain low temperatures The rise even occurre at temperatures at which there was an elevation of metabolism du to cold There was also a crossover temperature, at which exercise produced no body temperature change, which was, nevertheless higher for warm than cold acclimated animals This behavior of body temperature, which was characteristic in several species investi gated, supports further interpretation

The fact that there was no change in body temperature during exercise at the crossover temperature indicates that heat loss was equal to heat production only at this temperature Therefore only at the crossover temperature was the amount of work appropriately balanced to the insulation and environmental temperature At lower environmental temperatures, more work would have to be done and at higher temperatures less would have to be done to produce no change in body temperature Thus, it can be seen that graded levels of work could be done without change in body temperature by ad justing the work to the environmental temperature, just as was done by Erikson *et al* in tests on man (8) The dotted lines show these graded work levels which of course, fall to zero in the thermo neutral range The dotted lines also imply that insulation during graded work would remain the same at different temperatures but would be reduced by about 40 per cent below the insulation during rest

Horvath Is it possible that the work level is too high there, and that you are losing so much extra heat by these other mechanisms? That is, by the increased convection losses, you are losing more heat now than you anticipate? If you lower the level of work a little, it would reduce the speed of movement, and you might be able to maintain a constant level of heat production, in terms of heat loss

Hart I am thinking in terms partly of the work Erikson *et al* did (8) (see Figure 99 on p 295) where the work was graded to the en vironmental temperature He showed that as the temperature is lowered the amount of work being done by the men must be in creased to maintain a constant central temperature Thus, it would appear that more heat must be put into the machine during work to maintain a constant body temperature below a certain point than is required during rest

Davis Your peripheral vasomotor system changes with exercise, too, even in the cold You are getting more heat loss from that mechanism

Hart Yes by convection, and by greatly increased circulation

Davis Increased circulation and vasodilation
Fremont Smith And sweating
Hart All these mechanisms contribute to a decreased insulation which is represented by the broken line The change in the slope of the line relative to the resting state represents the amount that the body insulation is decreased and heat loss is increased
Burton May I point out that even in the process of shivering one is up against an inefficiency? Bazett and I showed many years ago (9) in the bathtub experiments that the insulation of the tissues between a warm and a cold bath went up about four or five times until one got to the point where the increased metabolism in shivering was elicited Then one lost this extra insulation and it dropped again to about only twice the dilation value
So even shivering itself is inefficient in this way I would say that you are showing that it is more inefficient in making the need for heat greater by producing more
Hart There is another point about these tests that requires emphasis, and this is even though the central body temperature may rise during exercise this does not inhibit cold thermogenesis
Keller It rises as a result of exercise
Hart Yes But we still have approximately the same amount of heat production from cold thermogenesis both before and during the exercise
Horvath This work activity you have here is relatively small isn't it just a little bit more than one and one half Mets
Hart I am glad you mentioned that The level of work is about two to three Mets but it was necessary to maintain this for a sufficient period for the measurements to be made
Horvath How long were these measurements?
Hart In these tests running lasted about three quarters of an hour We found in other tests (5) that this was not a maximum work level but we could not have the mice increase their metabolism to more than 50 per cent above this level and still have them maintain it We were interested in the level at which they could maintain a steady effort over a long period of time in this case about 30 or 45 minutes Figures 94 to 98 show similar effects on some other species

In lemmings (7) the lower line (open circles in Figure 94 is for resting animals the upper for working animals Here again constant work resulted in constant metabolic cost at different temperatures which was added to cold thermogenesis There was a rise in

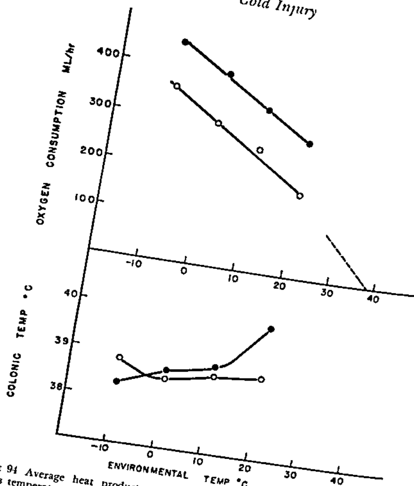


FIGURE 94 Average heat production (upper) and colonic temperatures (lower) at various temperatures during rest and work in lemmings

body temperature during 30 minutes running at high environmental temperatures and a slight fall at low, with a crossover temperature at −5°C

Thus, again, we have a tendency for the body temperature to rise slightly during exercise, but it does not seem to inhibit the cold thermogenesis

In our study on rabbits (7) (Figure 95), individual curves again show addition of exercise and cold thermogenesis down to −20° or −30°C when there appeared to be a metabolic collapse even though continued exercise was forced on he animals The effect of the exercise on body temperatu st evident in the cold

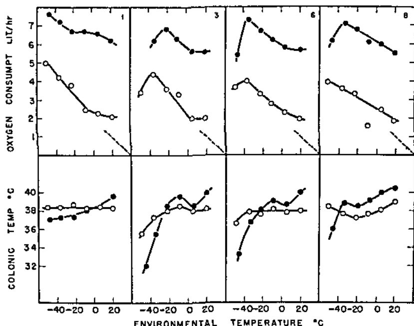


FIGURE 5 Oxygen consumption (upper) and colonic temperatures (lower) of four individual rabbits during rest (open circles) and work (closed circles)

with marked hypothermia resulting in some rabbits Evidently the amount of work prescribed was entirely insufficient to preserve heat balance and it is clear that the animals were much better off not exercising at all The crossover temperature for rabbits occurred at −20°C on the average so that exercise at lower temperatures was detrimental

This illustrates the point that the exercise is reducing the body insulation and increasing the heat loss to a much greater extent than it is increasing heat production, and the effect is sometimes detrimental to the animal

Burton Did you find the crossing over more marked in the smaller animals? I was wondering whether this might be very different in a large animal like man

Hart It has occurred in all the animals I have seen up to the size of a rabbit The only difference that I have noticed is in the environmental temperature level at which this occurs

In the smaller animals such as mice, it occurred at temperatures

about 0°C, while in these rabbits it varied all the way from 0° down to −40°C

Burton I was thinking not only of the change in the tissue insulation but in the insulation of the air caused by the movement I would think, from physics, this would be a very much greater effect in the small animal than in the large

Hart It should be much greater in a smaller animal

Burton So, in man this might not amount to this actual reversal of body temperature change

Hart I have not seen any comparable data for man, but I would like very much to discuss the human experiments on this later There are certain inconsistencies between the animal and human experiments which are hard to understand

Burch What happens to the circulation of the colon during exercise?

Hart I could not say

Burch Was there a shift of blood?

Horvath During exercise in man, I measured the blood in the area of the right and left hearts and the inferior vena cava I also measured it in the rectum at the same time As far as exercise is concerned those three temperatures rise at divergent rates, and rectal temperature follows along quite appreciably at a later interval It may lag as much as half a degree or even more than that, depending upon severity of exercise It will lag that much or more behind temperatures measured in the blood, at least in the thorax, where there is quite a lag Eventually there is a steady state The three temperatures will become equivalent again or approximately so, or there will be the same differential they had before exercise There is a terrific lag as far as the rectal temperatures are concerned on exercise

Burch That state is not steady?

Horvath This is not a steady state either The last three animals 3 6, and 8, have crossover points for body temperature on the negative side of the environment Associated somewhat closely to that point is the marked decrease in the oxygen consumption

Hart During exercise, yes, in those two animals That is related to an actual decrease in running speed At a certain level of hypothermia, some rabbits almost ceased running

Horvath Therefore that is the reason there is a huge drop in temperature

Burch When a thermocouple is placed in the rectum some heat must be conducted along the wire Was heat lost in that manner?

Hart It is not usually considered to be a major factor in cooling the animal in most experiments

Burton There is a much more important artifact in humans and that is the presence of large veins running near the wall of the rectum coming back from the legs During exercise one gets vasodilation and quite a marked drop can be found in the rectal temperature at that particular place

A study of the rectal temperature at different depths shows paradoxical drops through a certain region But I doubt whether it applies to colonic temperatures in animals

Hart In the rat to obtain good deep body temperature one must go in 4 or 5 cm

Burton One does not know whether the next subject has a different anatomical distribution

Horvath The higher up the better to get away from this possibility

Hart In all of these animal tests those exercising in the cold seemed to be able to draw on extra metabolism for cold thermogenesis even though the muscles are working at close to maximum sustained effort That is to say in rabbits and in some of the other animals the level of work is high in relation to what they can sustain over a long period of time Such an addition of metabolism would not be expected if cold thermogenesis were due to shivering An addition would not be expected but rather a replacement of shivering by exercise thermogenesis

Therefore it is important to determine whether or not any differences in metabolic patterns exist when the cold thermogenesis is due mainly to shivering on the one hand or to nonshivering thermogenesis on the other We can potentiate nonshivering heat production by cold acclimation In the next experiments we will examine what happens when we compare the effect of exercise on the warm and cold acclimated rats *

In these tests the oxygen consumption was measured first during rest Then the animals were placed in a treadmill and run at 1800 cm/min for about 30 minutes Finally there was another rest period Figure 96 illustrates the course of the changes in oxygen consumption and body temperature with time in warm and cold acclimated rats *There were two groups and they were both exposed to two different temperatures* 30° and −4°C

An alcohol lamp test was run here to show the pattern of response

*Hart J S Unpublished

of the apparatus to immediate changes in oxygen uptake of the lamp The lower portion of this diagram shows the average changes in body temperature of twelve animals The temperatures were not measured at the same time, in the metabolism and body temperature tests

Let us first consider the cold acclimated rats There is an increase of metabolism during exercise which amounts to about 5 ml per minute, both at 30° and at −4°C The starting point at −4°C was roughly equivalent to the exercise level at 30°C but the exercise increase was about the same In other words, the exercise did not substitute for the cold thermogenesis

In the lower section of Figure 96, we see that when the exercise occurred, the body temperature in the cold increased slightly However, the rise in body temperature did not inhibit cold thermogenesis

In the warm acclimated rats the situation is quite different for

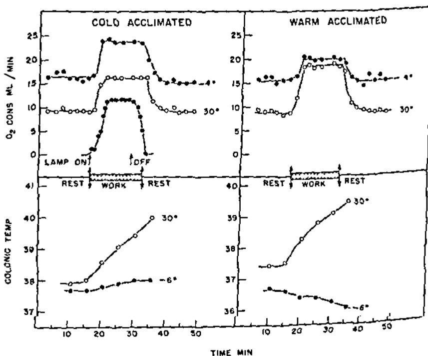


FIGURE 96 Upper graphs show oxygen consumption in cold and warm acclimated rats during rest and work at 30°C (open circles) and −4°C (closed circles) *Lower graphs* show colonic temperatures during rest and work at 30°C and −6°C *Alcohol lamp* shows time response of apparatus

it shows a very large rise in oxygen consumption at 30°C during exercise and only a slight rise at −4°C which hardly exceeded the exercise metabolism at 30°C. In addition a substantial hypothermia developed reminiscent of results with rabbits.

The interpretation of this is that in the warm acclimated rats which produce heat mainly by shivering the exercise is substituting for the shivering but not adding sufficient heat to offset the decrease in insulation and increased heat loss. Consequently we see a fall in body temperature during the course of the exercise. I might say in support of this that the level of metabolism reached in the warm acclimated rats at −4°C during exercise is lower than the level reached in the cold acclimated rats at the same exercise level which again supports the idea of the substitution taking place.

Burton: Would you define what you mean by "warm acclimated"? Is it very warm or what?

Hart: I am referring to animals which have been kept for 5 weeks at 30°C and in the cold they were kept at −6°C for a comparable length of time prior to the tests.

Pace: Did the temperature of the warm acclimated animals fall without the work?

Hart: They can maintain a normal colonic temperature at −4°C for the period of the tests.

Pace: But it wouldn't be as much as you have shown there at −4°C?

Hart: No.

Keller: To go back a little, why do you say that exercise does not inhibit cold stimulated thermogenesis? In the upper curve in Figure 4, warm acclimated for instance, there is a higher basal heat production at rest in the cold than in the neutral environment and then in the cold with exercise and at rest there is approximately the same maximal heat production.

Hart: That is correct.

Keller: The heat being produced at rest in the cold might be entirely inhibited and all of the heat that is being produced might be coming from exercise.

Hart: I believe that is what is happening, Dr Keller.

Keller: Why do you say the cold thermogenesis is not inhibited?

Hart: I would like to make a distinction here. As the same amount of mechanical work—in this case running—was being done, the same caloric effect of the work should be present at both 30° and −4°C. But since the total increase in heat production is much less in the

warm acclimated rats, we can only conclude that the cold thermogenesis itself is inhibited In other words, the shivering that occurred before is reduced by that amount But in the cold acclimated rats there was addition *of exercise to the cold thermogenesis, so that there* is no inhibition at all

Keller I don't see what your distinguishing evidence for that statement is

Hart That there is no inhibition?

Keller Yes I seem to have a blind spot there

Hart Because the rise due to exercise is the same at 30° and −4°C as has been previously seen in the other species

Keller *You are measuring total heat production without distinguishing* where that heat is coming from

Hart We cannot have substitution when addition is demonstrated

Burton Isn't Dr Hart *saying that in 'cold* acclimated animals there isn t a substitution of the work or exercise for the heat of shivering but in the warm acclimated animals there is?

Hart That is what I am trying to say here

Keller *I understand what you are saying* but I still don't understand why you say it

Pace I think this ties in with the thesis Dr Davis is trying to establish for nonshivering thermogenesis

Davis I think that still could be inhibited by exercise

Hart My point is it doesn't seem to be

Pace This would fit such a situation very well

Davis I think, taking into account the skin temperature changes that occur under exercise heat loss is going to be greater anyway

Hart Yes

Davis Exercise itself is not stimulated by the need for heat in the cold, and you don t show any skin temperatures on these during exercise

Horvath I think everyone is forgetting something If you look at the cold acclimated rats the level of metabolic increase due to exercise, either at the −4° or 30°C, is essentially equivalent

Hart Yes, that is correct

Horvath And in that case you have a definite rise in *the rectal* temperature of the cold acclimated low temperature exposure group On the other hand when you go to your warmer acclimated animals the level of metabolic activity as exemplified by the oxygen con

sumption is apparently equivalent in both cases when they are working.

Hart That is correct.

Horvath In other words, they reach equilibrium, but in that situation the rectal temperature or the colonic temperature as better expressed here is definitely falling in this warmer acclimated group. A distinction should be made regarding the total amount of heat which is being added to or subtracted from the animal. No one is doing this at the present time with your data. However, you are certainly right in what you deduce from it; there are two different responses there.

Burton In other words, the rats on the left hand side of Figure 96 must have been producing a great deal more heat than the ones on the right hand side.

Horvath They were.

Hart That is shown by the oxygen consumption.

Burton I meant, because the rectal temperature is going up during the exercise, they were producing a lot more heat than the rats on the other side.

Irving On the other hand, if you worked the two groups, the oxygen consumption was greater for the warm acclimated rats at 30°C and their efficiency was therefore less. Is that the implication?

Hart I do not know why that is so except that the warm acclimated rats were larger and hence were consuming more per animal.

Burch Would you think there was a difference in efficiency of muscle work between the two groups of animals?

Horvath This is suggested, but you should decide whether there is or there isn't. I would be tempted to say they are less efficient on the basis of the work required.

Burton Was the work a fixed number of ergs, or did the larger rats do more work?

Hart It was fixed speed.

Burton Then the larger rats are doing more work?

Hart Yes, that is what is complicated.

Hock You could express your units as milliliters per gram per minute or use some such weight reduction factor to help iron out the discrepancy shown.

Hart You could do that. I would not know what factor one should use, especially during exercise.

Burton You could express it in liters per erg of work in the two cases or gram per centimeter of work.

Hart You could do that

Burch Since the animals were larger, doing more work and using less oxygen, it would be greater This could be a possible explanation

Hart Figure 97 is an interpretation of what is happening in all of these tests During exercise at any temperature, there is a substantial reduction in body insulation, which means that a greater heat output is required for maintenance of heat balance than during rest This added requirement is illustrated on the left hand side of Figure 97 In a warm acclimated rat or in an animal producing heat in the cold largely by shivering, the exercise will partly or entirely replace shivering The exercise is shown here as entirely replacing the shivering Therefore, unless the exercise is sufficiently great to replace not only the shivering but the additional requirement due to decreased insulation hypothermia will result, as shown in the center of Figure 97

On the right hand side is shown the situation for a cold acclimated animal producing resting heat in the cold by chemical thermogenesis Exercise thermogenesis at the same level as that in the center diagram adds to the chemical thermogenesis to give the total heat output required by the reduced insulation If the total is greater than that required by the reduced insulation, body temperature rises until equilibrium is again established

Keller It does one thing it suggests that nonshivering thermogenesis isn t from muscle

Hart That is the next problem, and it is an interesting question

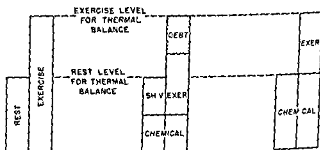


FIGURE 97 The left hand side shows heat production required during rest and work to maintain thermal balance the center shows replacement of shivering by exercise thermogenesis with resulting heat debt in warm acclimated animals and the right hand side shows addition of exercise to nonshivering thermogenesis with maintained thermal balance in cold acclimated animals

How can an animal which is exercising at a level that is perhaps not maximal sustained effort but close to it simultaneously add cold thermogenesis from the muscles? Where is this extra heat coming from?

Keller: I would say it would be rather presumptive, rather circumstantial evidence, that it isn't coming from the muscle, wouldn't you, when the muscle is exercising?

Hart: Yes, the muscles are exercising. On the other hand, cannot a muscle produce a basal level of heat production without contraction and at the same time be producing additional heat from the noncontractile process? During acclimation to cold, the noncontractile heat may simply be increased. I am just asking whether or not a single muscle fiber may carry out the two types of processes simultaneously.

Keller: All muscle fibers are not contracting simultaneously, of course. I suppose you could go back to the idea of two different types of muscle fibers.

Burton: There is some evidence that there are biochemical physiological differences between the character of the resting and the contractile metabolism of muscle. There is some work by Fisher of Toronto (10) about urethan separating resting and active metabolisms (of yeast). A considerable amount of evidence is accumulating that the maintenance metabolism may be of a different character from the exercise metabolism.

Pace: I think in hyperthyroid individuals muscle tissue shows an increased oxygen consumption, and this isn't due to shivering.

Burton: That is another example.

Pace: I don't see why there can't be complete dissociation between metabolic activity on the one hand and the special actomyosin function on the other.*

Hart: There appears to be no reason why these two functions can not be proceeding simultaneously.

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*EDITOR'S NOTE: Dr. Hart would like to add the following afterthought to his remarks at the Conference:

Evidence of dissociation of the contractile mechanism and metabolic activity due to cold is provided by the contrast in the effect of exercise and cold on blood lactic acid in deer mice (11). Figure 98 shows that when metabolism was elevated up to seven times basal by exposure of the mice to low temperatures there was no appreciable accumulation of lactate, while an elevation of only three times basal by exercise (running) resulted in an appreciable increase in blood lactate. It was also observed that exposure to cold did not further increase the oxygen debt contracted during exercise.

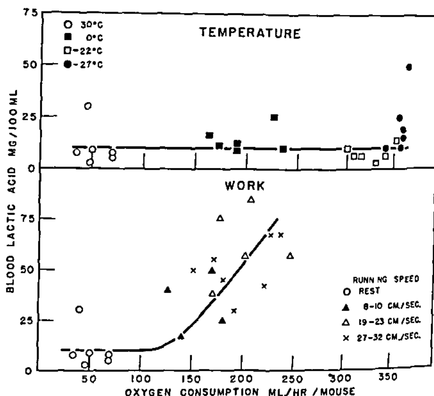


Figure 98 Comparison of the effect of exercise and cold on elevation of lactic acid in mice at various levels of metabolism Reprinted by permission from Hart J S and Heroux O Effect of low temperature and work on blood lactic acid in deer mice *Am J Physiol* 176 *452 (1954)*

Bass Is this increased heat production equally distributed throughout all cells in hyperthyroid patients?

Horvath Everywhere except the brain The brain and the central nervous system are not involved in this

Bass Would the liver be involved?

Horvath Liver is proportionally equivalent

Keller If that is the case and it was thyroid in origin the acclimated animal s basal metabolic rate ought to be higher when it is done in neutral environment

Irving You have not indicated in Figure 96 the elevation of the basal metabolism of the acclimated rats in a neutral environment Would the introduction of that factor add any further clarification?

Hart I don t think it would clarify the situation in this case

Carlson The significant increase is the increase in heat production with the cold stimulus

Keller And of course thyroid had too long a lag It has to be maintained

Burton This matter of the increased metabolic rate in neutral environment is much more complicated than appeared at first Dr Sellers early work (12) on it showed an increase If you look at the rest of the graph the activity declines to normal as time goes on doesn t it?

Hart That is correct

Burton I don t think we are suggesting this is thyroid but we were citing this thyroid action as an example of discriminating between resting and active metabolism of muscle

Keller Also if an animal that has been acclimated to cold is placed in a neutral environment his neutral environment is different than it was before acclimation As Dr Irving has shown in his animals in the Arctic it is necessary to redefine what the neutral environment is for the acclimated animal in order to obtain a true basal run

Burch In acclimation to heat it would be advantageous to develop a state whereby muscle work could be performed without generating too much heat while in cold environment it would be advantageous to develop a state whereby more heat is produced for the same amount of work

Do you think your data tend to support such concepts? This may represent efficiency or differences in metabolic pathways It may be possible to shift from one pathway to another and become more or less efficient with muscle work?

Hart You are thinking in the biochemical sense?

Burch In the physiobiochemical sense

Keller You could determine that by doing myograms on isolated muscles couldn t you before and after acclimation?

Pace How do you define efficiency ?

Burch This could be considered to represent the ability to do mechanical work with less oxygen or metabolic cost

Pace I think this is a rather dangerous use of the term efficiency although it is a classic term It doesn t make too much sense because for example you can perform work without any oxygen anaerobic work that is infinitely efficient

Horvath You have to pay it back eventually If you pay this back you measure its cost in terms of the eventual energy

Bass: In connection with acclimation to heat, I believe Robinson (13) demonstrated that as men became acclimated, their energy cost for doing a specific task seemed to go down somewhat. I don't know whether this has ever been confirmed.

Horvath: It was confirmed here at the Fort Knox Laboratory in 1943.

Fremont-Smith: To go back to the earlier discussion, isn't the nerve another example of two different kinds of metabolism? Nerve firing certainly has different metabolism from the condition in which the nerve is not firing, which also has a metabolism. I think you have another perfectly good example. Actually, you will probably find three or four different levels of separate kinds of metabolism that cells can exhibit which are relatively independent of one another.

Burton: There is the work of Lilienthal, before he died, with Zierler (14) on mammalian muscle, which reveals (this is still being argued about, but I can't see any way out) that if a muscle is stretched, its metabolism decreases greatly. The oxygen consumption drops to about one third of the value, as long as you leave the weight on. We are beginning to entertain the idea there are a great many factors which can change this "maintenance," nonactivity metabolism of muscle, which we may not know about.

Hart: Noncontractile?

Burton: Noncontractile.

Davis: A. V. Hill (15) has shown this in hypotonic Ringer's solution, where he can stimulate the muscle and not have it contract but can raise the heat production in the cold by a third of the amount that it was producing before, when it was contracting.

Keller: In the presence or absence of electrical activity?

Davis: He felt there were electrical responses going down to the muscle but, instead of contracting, they produced a chemical type of heat production.

Burton: Then there is the latest work from England, by Katz (16-22), showing that at synapses there is a continuous release of a small quantity of acetylcholine, probably coming from individual cell organelles. Only when released all at once does it excite the action current of the next nerve. This continuous subthreshold activity might again change the oxygen consumption.

Pace: Also the dinitrophenol effect. Certainly, oxygen consumption can increase tremendously without increased contractile activity.

Hart: Cahn (23) showed that chemical changes suggestive of these effects occurred in muscle after dinitrophenol administration.

Fremont Smith The cell is a factory for several units which have some degree of independence One may be going fast and another slow We have been talking as if the cell were a single unitary function It is multiple at least

Burton The latest work from England (16 22) might even suggest that if we could use microelectrodes in the muscle or the neuro muscular juncture of the acclimated rats we might find there was a greater release of these quanta of acetylcholine yet insufficient to produce contractions unless this release is all at once as when an impulse arrives from the nerve

All we are doing is speculating here and opening our minds to the possibility of this resting metabolism being a very variable thing

Hart We have been discussing the implications of another charac teristic of nonshivering thermogenesis namely that it is added to the effect of exercise I would like to conclude by inquiring whether or not this characteristic might be useful in assessing the occurrence of noncontractile cold thermogenesis in man by measurement of the increased energy cost of exercise in the cold

Table V tabulates some of the results that have been made on man At the outset it should be mentioned that in the animal studies un like those on man there is already a very efficient thermogenesis in the cold Heat production in rats increases within 3 minutes of ex posure to the level required to maintain normal body temperature (24) In humans there is often a lag in shivering and there is a con siderable heat debt Therefore exercise is superfluous for maintain ing temperature in the cold for rats but a valuable asset for man

Another effect which is shown by some of the older data is that there seems to be a difference between the animal and human studies in the way heat production is related to temperature during rest and work

In Rubner s (2) studies cited by Lusk (25) total heat production per hour during work was practically independent of temperature as also was that in Nielsen s working men (26) One of the characteris tics of their study was that bicycling men with practically no clothes on showed a constant heat production during work with a variation of environmental temperature from 5° to 35°C This is a very remark able thing when you recall that in their studies the rise in temper ature produced by work was also practically independent of environ mental temperature

This seems to be in marked contrast to the results we have ob tained on animals and indeed it is very hard to reconcile from the

TABLE V

Energy Cost of Work at Different Temperatures (in Man)

Clothing and Type of Work	Temperature (°C)	Heat Production		Rectal Temperature		References
		Rest	Work	Rest	Work	
		gm CO_2/hr				
	+ 7 4	—	84 0	—	—	
—	+15 5	—	84 5	—	—	(25)
	+25 0	—	78 4	—	—	
		cal/hr				
Light	+ 5 0	—	550	—	Rise independent of temperature	(26)
Light	+20 0	—	550	—		
Light (Bicycling)	+35 0	—	550	—		
		cal/M^2/hr				
Herringbone	+24 0 (1 hr)	—	207*	—	—	(28)
Twill	− 3.3	—	202	—	Rise 0 7°C	
Coveralls (Walking)	−46 7	—	222	—	Rise 0 2°C	
		cal/hr				
Light	+25 0 (2 hr)	136	360†	—	—	(27)
Arctic (Walking)	−29 0	208	451	Slight fall	Maintenance	
		cal/M^2/hr				
Shorts	+20 0	36	107**	—	—	(29)
Shorts (Bicycling)	+10 0 (1½ hr)	46	135	Slight fall	Maintenance	

* Exercise started upon entering room
† Average of tests during continuous 8-day exposure
** Exercise started after shivering was seen

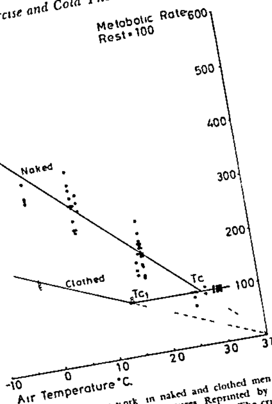


FIGURE 99 Heat production during work in naked and clothed men for maintained thermal balance and comfort at various temperatures Reprinted by permission from Erikson H Krog J Andersen K L and Scholander P F The critical temperature in naked man *Acta physiol scandinav* 37 35 (1956)

physical point of view when you have an increase in the gradient of some three to fourfold with a constant heat production a constant rise in body temperature and a constant heat loss I have never been able to understand this *

Irving Didn t they indicate an explanation in the diminished skin temperature because the gradient from center of the body to skin increased with the cold and the insulation therefore was greater? When related to the external insulation that relation

* The results of Nielsen are also not consistent with recent findings of Erikson *et al* (8) These data presented in Figure 99 show that the heat output of men during exercise must be increased to maintain subjective comfort and constant core temperature as the gradient from body to air increases

changed also, indicating that there was the two step change in both steps of the insulation, from body to skin and from skin to air

Hart Yes, that is right The skin temperature was related very directly to environmental temperature in those experiments Would that be sufficient to explain this constancy?

Irving I don't think they made that as a conclusion but they pointed out that that was of probable significance in the preserved heat economy of work production In fact, I think they said that it indicated that such changes had to take place if the equal economy of work production was maintained

Horvath Exercising people in the nude at −60°C, we were able to maintain their body temperature with a large expenditure of calories, around 400

Hart That was higher

Horvath This is in the nude It is a little bit higher in the Arctic clothed group At −29°C, they wore Arctic clothing Wouldn t that raise the metabolism, because of the work of carrying that clothing?

Hart In your experiments you had a somewhat greater energy cost of work in the cold (27,28)

Horvath That was explained very simply because of the extra amount of work they had to do If you ran them at −30°C in the nude and −60°C in the nude their caloric production was essentially the same as it was at +25°C

Hart In Dr Carlson's (29) experiments, he apparently concluded that the energy cost of work in the cold was somewhat greater than at room temperature Was that right, Dr Carlson?

Carlson Yes, I believe Dr Horvath's (28) work has such indications also

Siple Is this based on the fitting the type clothing or something of this sort?

Horvath It was herringbone twill Actually, the values are about the same There is a slight increase

Carlson I should like to point out that we (29) did have information about heat loss The skin temperatures with the 10°C exposure resembled the skin temperatures reported by Horvath (27) at −40°C with clothed subjects

Henschel Could the slight increase in energy expenditure be due to increase in respiration?

Horvath Just what you expect, on the basis of minute volume, which is roughly about 29 liters per minute

ple Do you possibly have any sweating?

orvath There is no question they were sweating in Arctic cloth This is essentially working in a tropical environment We asured the weight loss

iple When I was doing about 2 to 3 hours of heavy work in a 0°C environment day after day I could never adjust to keep from eating I got down to wearing what I would call light Arctic othing

Horvath You were still sweating?

Siple At −60°C I would even take off the hat for awhile and I worked in just mitt liners For the first hour I couldn't keep from sweating

Fremont Smith Did you ever try grading your exercise gradually increasing it?

Siple I was keeping up a fairly regular pace if I stopped I got cold

Fremont Smith I wonder if by increasing your exercise step by step you might not have found a rate of increase in exercise that would have prevented your sweating and maintained your warmth

Horvath The problem was simply that in order to prevent sweating at −60°C while working at 350 to 400 calories the men had to take off most of their clothes in fact all of them

Siple And if even your ears were exposed you would soon suffer frostbite It was necessary to keep the skin at least superficially covered to prevent frostbite

Horvath We didn't know as much about frostbite as you did

Siple Maybe it is simply a matter of fatigue that brings sweating to a stop As I kept on working between the second and third hour I couldn't sweat no matter how hard I tried to work

Henschel Your rate of work did decrease

Siple Maybe it did decrease I think that at least the body efficiency changed At the end I produced six bags of snow 100 pounds each per hour which was about the same rate as at the beginning

Horvath Did you knock the snow off the walls first and pick it up in the second and third hours?

Siple It was done in periodic sequences The loss of stored heat from the body particularly over the solar plexus and kidney region took hours to pay back Those parts of the body felt cold to the touch even after 2 to 3 hours in a warm environment Actually climbing up stairs out of the mine was the hardest work of all At times on

the way up after being tired out my nose frosted Going down I never frosted my nose but coming back I did a number of times Stirring up the air may have increased the wind chill

Fremont Smith Isn t there some evidence that the sudden commencement of heavy work will initiate more sweating than moving into heavy work gradually? I am aware of this as a natural observation on myself

Horvath It depends on how much clothing the subject is wearing

Fremont Smith For any given rate of work and amount of clothing the faster you move into heavy work ?

Irving In comparing the results of studies of animals and man we forget that clothed man is no longer an animal because he is wearing an insulator which is inelastic inflexible and invariable with the exception of such small openings as are permissible Then as you say the ears and fingertips and so on will freeze I have noticed that even Eskimos cannot accommodate their clothing in spite of their long training and experience so that they can do heavy work in thick clothing without sweating They just can t get away from it Their skill and their clothing are probably nearest to perfection It just shows that our invariable clothing is not comparable with the variable insulation possessed by an animal Therefore the region in which the metabolic response of man to his environment will be natural or normal either to man as an animal or comparable with other animals will be in that area of temperature in which the man can become normally and comfortably adjusted to work practically naked That means then that we can scarcely expect normal regular metabolic responses to changing temperature from man except in that very limited narrow range of temperature to which man can become comfortably adjusted in his naked skin

This is of course the basis on which Scholander (30) proceeded to carry his experimentation to the naked aboriginal people not because they were aboriginal or different in that respect but because they were accustomed to running around naked

Hock The sweat rates in Eskimos are higher than in Caucasians under comfort zone conditions or warmer At body heat debt conditions no difference in insensible perspiration was found This greater sweating is due to the increased heat production with consequent higher skin temperatures and higher sweat rates in the Eskimo compared to the Caucasian I am quoting Rodahl and Rennie (31)

Keller Isn t there an observable variation in individuals?

Hock Yes but the sample is too small to allow any conclusions on this subject except to say that individual variation was probably less and certainly no greater than the Caucasian variation

Irving Relative to your remarks Dr Siple I think perhaps we should also consider in relation to your apparent deterioration in sweating ability if that is what it was or the gradual cooling that by the accumulation of sweat in your clothing its insulative qualities probably diminished considerably

Horvath Also fatigue of the sweat glands That may also have occurred

Hock Were you getting dehydrated? You were not drinking water

Siple Not while we were down in the snow mine The pattern of sweating followed by cooling repeated itself so regularly without change in the work output I got the impression that I might be getting more efficient work with less production of heat in my body

Bass Something to be taken into account is that a person starts to sweat at a lower skin temperature when he exercises than when just resting in the heat That is correct isn t it?

Horvath Yes

Bass This skin temperature at which sweating starts is lowered as the person becomes acclimated to heat It may also be lowered as the person becomes physically conditioned

Horvath It is

Bass This might be a factor in the sweating experiment by Dr Siple If you start sweating at a lower skin temperature it may be that your sense of physical comfort won t permit you to get that low a skin temperature until the work you do

as well as the strenuous work You had to keep working or you got too cold Although there were short rest periods the rest periods had to be fitted in between catching one s breath and keeping from getting cold

Bass What is the effect of high altitude on sweating thresholds?

Talbott I don t think there is an appreciable difference It is essentially the amount of work one is doing

Horvath Really the basic thing Dr Irving was trying to bring out relates to something else I don t think you meant to say We talk about work both in man and animals and fail to give levels or in

tensity of this activity We have a good scheme that Dr Dill proposed several years ago for levels of men in terms of basal metabolic measurement From 2 to 4 Met is equivalent to light work 4 to 8 is moderate work and above 8 Met is severe work

You said the metabolic activity of these animals was approximately 3 Met and they could not do this work for any appreciable period of time

Hart It is close to maximum sustained effort

Horvath So that 3 Met in these animals is apparently equivalent to above 8 Met in the human This is what I think needs to be clarified in order for us to get a clear cut transference of concepts from experiments on animals to man

Also in terms of what Dr Siple said it is hard to say how much work he actually did He dug out so many bags of ice which weighed 100 pounds apiece but he might have been able to get one chunk of ice with one stroke and the next time it might have taken five or six Such factors should be considered

Siple There is one criterion From the mine to the surface was 90 feet Climbing these stairs would probably result in an energy expenditure at a level of 11 Met This was a 20 degree slope at least So we could probably establish a fairly close estimate of the actual work output

I would assume I was doing at least 6 to 8 Met of work during the whole time I was down there with brief pauses to catch my breath and still not get too cold

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ENERGY BALANCE IN COLD ENVIRONMENTS

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Headquarters Quartermaster Research and
Engineering Command U S Army
Natick Mass

My topic for discussion is caloric balance in a cold environment In order to cover all my material I am arbitrarily presenting certain conclusions and anticipate that the details will be developed consequent to the questions Energy metabolism or caloric expenditure everything else being equal is increased in cold exposure However the basal metabolism when measured under the criteria of this test is not altered when the individual is exposed to cold Resting energy expenditure measured under conditions where cold exposure is present does increase when compared to the standard metabolism at 25°C It should be emphasized that the differences noted here are related to the technical requirements of the two methods of measurement

The energy cost of doing a specific amount of work in cold environments is higher than it is when doing the same task under conditions of normal ambient temperatures It is generally agreed that this is because more clothes are being worn the hobbling effects of clothing the weight of carrying the clothing around the difficulty of the terrain—snow ice slipperiness—are all things that go into making a less efficient operation

Fremont Smith Are naked persons less efficient in the cold?

Henschel No

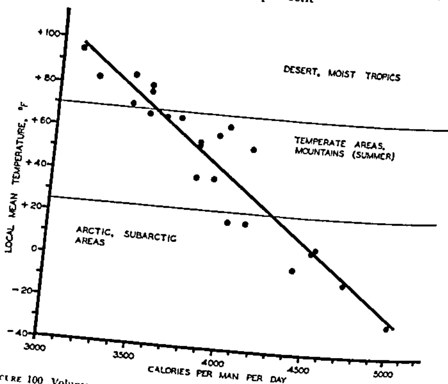
Burton It seems to me you are shifting your ground Now you are assuming that a man wears more clothing when in the cold When you start off by saying that the energy exchange went up in the cold you are asssuming other things were equal he did not change his clothing

Henschel Allow me to present some of the data on which my con

clusions are based Figure 100 comes from Johnson and Kark's (survey of caloric intake of soldiers during the war, 1941 to 1945 different places in the world, from hot environments to cold enviror ments The data fall nicely on the line, but several questions remai unanswered The data are based on mess surveys which tell pri marily what has been cooked, but they don't show what the men ate These subjects were not weighed periodically over a fixed length of time to determine whether they were in or out of caloric balance Furthermore, there was no real knowledge of their work output

Carlson In behalf of Johnson and Kark, I would like to say that they measured food intake better than you say they measured what was left on the plate

Henschel From a large group You measure what is cooked and what is thrown away and divide by the number of men They are probably within plus or minus 10 per cent



ICURE 100 Voluntary caloric intake North American Troops Reprinted by per ission from Johnson R E and Kark R M Environment and food intake in man *ience* 105 378 *(1947)*

Burton It is a very valuable graph but the meaning is not as simple as it appears

Henschel That is right Table VI breaks down the available information These figures show a good progression in level of caloric intake from cold on the top to warm at the bottom the caloric intake decreased as the temperature went up I have recalculated these values on the basis of calories per kilogram of body weight per day Musk Ox was about 60 kg cal /kg and the two Pacific areas were approximately 50 kg cal /kg The data in Table VII suggest that the caloric expenditures from the above studies may be somewhat too high Dr Kark s Prince Albert studies (2) show that these men were quite active They were out from 8 to 12 hours a day and traveling overland on foot they took their major equipment with them actually covering 150 miles in 10 days in each of those three periods The mean temperatures for the 10 days are given in Table

TABLE VI

Total Calories and Composition of Diet Eaten by Soldiers Operating Under Various Environmental Conditions

Place and Troops	Environment	Average Body Weight (kg)	Average Caloric Intake/Man/Day	Percentage of Calories provided by Protein	Fat	Carbohydrate
Canada mobile force Musk Ox	Arctic and subarctic	73 0	4400	11	40	49
U S A ground troops	Temperate	69 0	3800	13	43	44
Colorado Rockies infantry	Temperate mountain (9000 ft)	69 5	3900	13	34	53
Pacific Islands ground troops	Tropics	70 0	3400	13	33	54
Luzon infantry	Tropics	65.5	3200	12	34	54

Reprinted by permission from Johnson R E and Kark R M Environment and food intake in man *Science* 105 378 (1947)

VII The second period was fairly cold A mean temperature of −16°C is fairly cold, and 150 miles on foot over Arctic terrain in the wintertime is hard work These men consumed between 4000 and 4500 kg cal per day In this situation, there was control, because this was a small group out on the trail and exactly what was happening to the men was known This is the exact caloric intake level

Dr Molnar s (4) data, which were obtained at Fort Churchill, Canada, in 1948, show about 3600 calories per day intake at 0 6°C His men were out for 5 or 6 hours a day, at hard, physical work

Ladd Field, Alaska, was very cold, so the men stationed there were out during the daytime only They covered anywhere from 3 to 8 miles overland on foot in the cold

Swain's (5) figures from his work at Fort Churchill in 1947 are higher than anyone else s in spite of the fact that his men were not doing the type of work that the men at Prince Albert were doing nor were they doing nearly as much

Rodahl's (6) data in Table VIII are from Air Force personnel caloric intake at Ladd Field was 3200 Our data (7,8,9) on QMC per

TABLE VII

Summary of Food Intakes During Several Cold Weather Studies

Location	Temperature (°F)	Caloric Intake	References
Prince Albert, 1944 (I)	20	4000 to 4150	(2)
Prince Albert, 1944 (II)	−3	4600 to 4700	(2)
Prince Albert, 1944 (III)	9	4450	(2)
Fort Churchill, 1948	−33	3390	(4)
Musk Ox, Canada	39 to −18	4400 to 4500	(3)
Ladd Field 1943	−18	3200	
Ladd Field, 1950	−25	3850 to 4500	
Fort Churchill, 1947	16	5290	(5)
Fort Churchill, 1947	−20	5650	(5)
Fort Churchill, 1947	0	5250	(5)

TABLE VIII

Summary of Caloric Intakes for Troops in the Arctic

Temperature (°C)	Calories per Day	Activity	Method	References
−17	5650	Garrison	Mess inventory	(5)
−20	5000*	Vehicle travel	Mess inventory	(7)
0	4400	Vehicle travel	Mess inventory	(3)
−10	3733	Garrison	Servings (plate waste)	(8)
	3200	Garrison	Servings (plate waste)	(6)
−15	3600	Garrison	Servings (plate waste)	(9)
−23	4600†	Bivouac	Servings (plate waste)	(9)

*Not corrected for body weight loss
†Corrected for body weight loss

sonnel engaging in a garrison type of activity showed a 3600 kg cal intake when the men were out on bivouac These men slept warm but were out in the cold during the daytime

These figures were corrected for body loss The men were actually doing heavy physical work and they were on the move all day They carried all equipment with them including tents They got up in the morning picked the tents traveled 12 miles overland pitched camp and cooked their own meals

Morning and evening weights were taken urine specimens were collected and all fecal material was collected All intakes were measured and any food not eaten was measured every day We took also a sampling of energy expenditures for various types of activity during this whole time

Horvath What was actually the base level of caloric intake when you subtracted the actual cost of work? That would compare a little more precisely with what went on in the garrison

Henschel At the time, they ran around 3100 kg cal before they took to the field. Permit me to show you some additional data. In our first study (Figure 101), the individuals during the pre-bivouac time lived in the barracks. This is for a 10 day period of which they were out 5 hours each day doing physical work in the cold. They carried a light type of equipment pack with them, either pulling it on sleds or carrying it. They were also out for 5 hours during the bivouac period. In the bivouac area the men moved into tents outside the barracks. The tents were heated in the daytime but unheated at night. The post bivouac period is when the men came back into the barracks again. Each period was about 10 days in duration. This group was being fed a five-in-one ration in any amount they wished. It was measured and served to them on plates, and it was weighed when it went on the plates as well as when it came back. Also samples of what was refused were taken for analysis.

Figure 102 shows actually what happened to the caloric intake

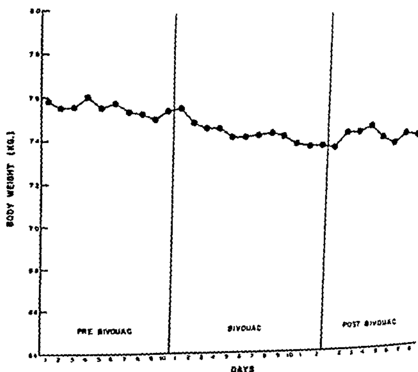


Figure 101 Body weight changes during subarctic bivouac

During the time when the men were in the barracks when they first arrived at Fort Churchill their intake increased a little However when we took them out into the cold for the first 3 or 4 days their caloric intake decreased but then it increased again In general the men have maintained their weight fairly well on a caloric intake of 3500 During the actual bivouac period intake was 3600 kg cal

Horvath They lost the most weight when they were eating the most That was probably because you worked them harder

Henschel Figure 103 and Table IX compare four situations Fort Churchill Yuma Natick and Mt Washington Table IX also shows outdoor ambient temperature differences At Fort Churchill the temperature averaged −26°C for the whole period At Yuma the experiment was run between 3 and 4 weeks so actual control and

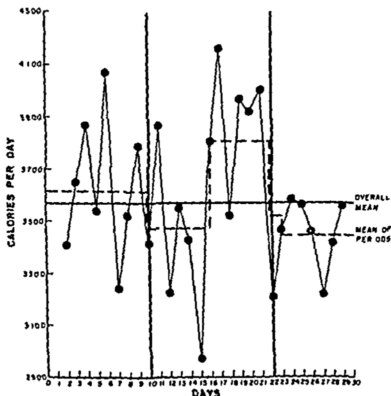


Figure 103 Caloric intake during a subarctic bivouac

TABLE IX

Weather Conditions Encountered During Cold, Temperate, and Hot Climate Bivouac

Site	Outdoor Ambient Temperature	Room Temperature	Relative Humidity
Fort Churchill	−25°C (−15°F)* −16°C (−13°F) to −38°C (−36°F)†	22°C (71°F)* 15°C (59°F) to 25°C (77°F)†	40* 25 to 60†
Yuma	34°C (94°F)* 27°C (81°F) to 43°C (110°F)†	34°C (94°F) Tent* 27°C (81°F) to 43°C (110°F)†	39* 18 to 81†
Natick	22°C (72°F)* 18°C (65°F) to 27°C (81°F)†	22°C (72°F)* 18°C (65°F) to 27°C (81°F)†	70* 46 to 95†
Mt Washington	−4°C (25°F)* −4°C (25°F) to 4°C (40°F)†	23°C (71°F)* 20°C (68°F) to 31°C (88°F)†	45* 30 to 70†

*Mean
†Range

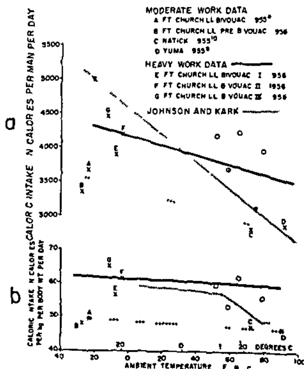


FIGURE 103 Relationship between mean daily outdoor ambient temperature and mean daily caloric intake at two levels of work Reprinted by permission from Welch B E Buskirk E R and Iampietro P F Relation of climate and temperature to food and water intake in man *Metabolism 7 14 (1958)*

exposure were for that length of time the mean temperature was 34°C for the entire period At Natick the temperature was 22°C On Mt Washington it was −4°C

Burton Don t you think you should report the wind velocities as well? They are very different in those different locations

Henschel You are right

Burton There would have been a very high wind at Fort Churchill

Henschel The wind was probably 10 miles per hour at Fort Churchill and from 5 to 8 miles per hour at Yuma The men were in the woods well down into the timberline on Mt Washington so there would have been little wind there There would have been little at Natick too

In all of these situations, the men lived on C ration and they were allowed to eat as much as they wanted If they wished they were given an individual assault ration or trail ration as an extra There was a rule that the men had to eat their C ration before they would be given anything else

In Figure 103 we have tried to summarize our own experimental data at Fort Churchill at Natick and at Yuma at Fort Churchill the men did moderate work and heavy work heavy work being 12 miles of traveling per day carrying everything they owned with them The moderate work was the bivouac where the men didn t carry much and where they lived in tents but worked outside every day Yuma and Natick we considered also as falling into this moderate work category

Figure 103 *A* shows calories per man per day as against temperature Our figures range from hot at Yuma down to cold at Fort Churchill There is heavy work and light work the differential between them being the difference in the level of work As is evident caloric intake per man per day decreased as the temperature increased Figure 103 *B* shows Johnson and Kark s line (the little hatched line that comes down at a steeper angle) On the bottom part of Figure 103 we have plotted intakes in terms of calories per kilogram of body weight per day In terms of calories per kilogram per day these are pretty well straightened Actually when Kark and Johnson s material was similarly treated it straightened out their line quite well except for one dip in the temperate region The men in Luzon and the Philippines were active combat troops and the results were entirely different from those of the others the garrison troops

Burton Is the heavy line shown in the upper graph in Figure 103 the regression statistically significant? You have a presumably statistically calculated regression line

Henschel That is right

Burton I am wondering if the open circles belong to it

Henschel Those open circles are data taken from some of Dr Adolph s work

Burton Have you the P value for the validity of that line?

Henschel Do you remember seeing it Dr Bass?

Bass No

Henschel Anyway I think the basic conclusion is that caloric expenditures in cold environments or any other conditions are not

as high as common concepts have led us to believe and they rarely exceed 4500 calories a day.

Blair: What was the estimated number of calories you had for the Pole Station, Dr. Siple, do you remember?

Siple: I think perhaps it was not more than 4000 to 5000.

Hock: I would like to call attention to the work of Drury (10) who found that the Eskimos living as nearly normal a life as is still possible in Alaska have a mean daily caloric intake of 2867 kg cal over a year's period with a variation from about 1250 to 4000 kg cal daily, probably depicting periods of food scarcity and abundance. There may be no more active people in our own civilization than these hunters. Seasonal differences in caloric intake were small in this group and Drury felt that they were not related to nutritional requirements.

Taylor: Is the weight constant over this period?

Hock: Food is not always available as it was in our experience.

Taylor: Over the course of the winter, do the men maintain their weight?

Hock: Weight gain and loss are not detailed in this study. Again it would depend on food availability.

Burton: I would like to make one point in the interpretation of Johnson and Kark's findings. True, we should have corrections for large weight loss. Also recognizing that the wastage wasn't measured and the factor of variable work, I still think that the graph is very important in that it is an index of human behavior in different conditions. Whether it is entirely a matter of stimulation of appetite or whether it is that people have to work harder to do a given job in the cold, this remains a very useful statistical correlation as to the behavior of the subjects. Even if they wasted a lot of food, it means that they were stimulated in some way to take more food on their plates. This is a very important clue to a physiological change induced by the different climatic conditions on human behavior.

Henschel: Table VI gives the composition of the diet. It shows what men eat when given a choice (1). Actually there was no difference in the composition of the diet. The proportion of fat, carbohydrate and protein was the same throughout in all of the studies. Ours in the bivouac, where we had used the five-in-one ration and allowed men to eat what they wanted, bears this out.

Carlson: Do you consider this a reduction of fat intake?

Henschel: No.

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Henschel That is right

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Carlson Do you consider this a reduction of fat intake?

Henschel No

TABLE X

Relationship Between Food Composition and Caloric Intake During Arctic Bivouac

		Percentage of Total Calories as		
	Total Calories	CHO	Fat	Protein
Prebivouac	3614	48 04	38 21	13 75
Bivouac No 1	3476	45 91	40 42	13 67
Bivouac No 2	3821	47 54	38 37	14 09
Post Bivouac	3449	48 51	36 82	14 67

Carlson There seems to be a large caloric difference from the percentage figure

Henschel Johnson and Kark make the statement that they did not consider there was any difference Table X is from our own data where the caloric intake varies somewhat between the groups Again, the men took as much food as they wanted, and the composition remained constant

Pace I would like to ask how valid the figures are for North Woods workers, for whom values of up to 7000 and 8000 kg cal per day have been estimated

Horvath That has been quite well shown by Benedict (11) If you worked 14 hours a day at the kind of work some of these men did it is quite conceivable you would eat that much I was always served the same amount of food as the men, and I ate only a small portion of my serving, but they ate all of theirs

Hock Eskimos do not derive any higher percentage of calories from fat than we do They derive about 35 per cent, as in our diet However, 30 per cent of the calories are from protein, and 32 per cent from carbohydrate, which is much different from our diet Again I am quoting Drury (10) Rodahl (12) found Air Force troops in Alaska consumed a mean of 2950 kg cal daily, of which 13 1 per cent were protein, 37 5 per cent fat, and 49 4 per cent carbohydrate

Siple Is any excess eliminated?

Hock There are great swings in daily consumption Drury found variations from 5 per cent of calories derived from fat to 85 per cent

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Horvath Would you comment about feeding schedules?

Henschel On a feeding schedule at −37°C sleeping in an Arctic sleeping bag if a man eats 500 kg cal just before going to bed he can keep his toe temperature higher for 5 or 6 hours and his rectal temperature higher throughout the night than if he had not had the meal

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METABOLIC AND ENERGY BALANCES OF MEN IN A COLD ENVIRONMENT*

DAVID E BASS
Physiology Branch
Headquarters Quartermaster Research and Engineering Command, U S Army
Natick Mass

THE RESULTS OF THIS CONFERENCE show quite clearly that man in a cold environment is not necessarily a cold man I would like to report the results of some studies done on men who were demonstrably cold (1,2) I shall borrow a page from Dr Siple and Dr Henschel by starting with my summary and conclusions

We studied men who were living nude at 15 6°C for 12 consecutive days The questions we asked ourselves were not those which are the most logical to ask, in terms of cold stress, such as What happens to skin temperature rectal temperature oxygen consumption and so on We were mainly interested in the more ubiquitous bodily balances, those general metabolic balances which inevitably must support any physiological changes resulting from stress We therefore measured nitrogen phosphorus potassium chloride, and water balances There is one other balance we studied simultaneously in men exposed to cold under metabolic ward conditions *i e* controlled energy balance We also attempted to assess adrenal cortical function and body composition mainly with reference to body fluids

I shall list our conclusions There is nothing dramatic about them but the data show the following picture

Under conditions when men are really chilled for a large percentage of the time, *i e*, for as long as 12 days, there is a marked increase in turnover of food and of energy expenditure This of course, is well known The food intake increased approximately 25 per cent, and the energy expenditure increased approximately that

*The data on metabolic balances and acid base balance presented here have not hitherto been published The research was performed with the collaboration of C R Kleeman and M Quinn Thyroid function studies described here were carried out by S Ingbar The omission of quantitative data from these later studies is deliberate since Dr Ingbar is preparing detailed papers for the open literature

much in this situation These incidentally are inactive sedentary men

However despite a marked turnover in input and output the balances are essentially unchanged We found no evidence of any catabolic response and the body does very well It just maintains balance The body also maintains its core temperature very well in this situation

These changes or rather lack of changes occur in the presence of what might be called an increased thyroid balance The thyroid gland increases its uptake of iodine and increases the rate of formation of hormone and rate of release At the other side of the thyroid balance there is an increased peripheral utilization of circulating thyroxin

We attempted to assess adrenal cortical activity and found no evidence of increased activity by the admittedly crude methods we used These were eosinophil counts and 17 keto steroid excretion There was very little change in acid base balance I will present some data which are suggestive though not statistically significant that there may be an interesting alteration in renal response to acid base requirements

We performed two studies In the first (3) five men who were volunteer soldiers lived in a temperature regulated chamber for 7 weeks They did not leave the chamber except on three occasions to take showers For the first 3 weeks the room was maintained at about 26°C This was our control period Then the room temperature was dropped to 14°C with about a 3 mile wind The men lived there for the next 12 days Recovery period followed that—2 weeks—again at about 26°C

The activity of the test subjects was constant throughout all these periods They led a sedentary existence During the cold period for 4 hours in the morning the men reclined on beds wearing shorts and socks For 4 hours in the afternoon they again reclined in their beds but were permitted T shirts In the evening they were permitted to walk around the room and visit with each other during this time they were permitted to wear only T shirts and at ten o clock they were given a blanket and retired We have found that it is almost impossible to maintain men in essentially a nude state for 12 days under these conditions without permitting a blanket during the sleeping hours In summary then our men were chilly for 16 hours of the day during the cold period Lest some of you think 14°C is not chilly I will show some examples however I am sure most of you realize that it is cold



FIGURE 104 Typical posture of test subject during cold period

Figure 104 is an unposed picture of one of our subjects in a typical posture His T shirt was well fitting and reached down to his waist As is evident he tried to avoid the cold but wasn t too successful

Figure 105 illustrates one of the informal attempts to rewarm Soon after we saw this happening, we instituted a new rule no more than one per bed

The men tried all sorts of dodges There was a reading lamp on the bed so one fellow suddenly became an avid reader We found he was trying to read under the blanket late at night We then did away with the reading lamps

Pace Are these men nude or lightly clothed?

Bass They are nude except for shorts, socks, and the T shirt

Pace So, they are lightly clothed

Bass If you want to call it that In the second study I shall de scribe, they are wearing just shorts

Blair When exposed to a 3 mile wind, whether they are clothed or unclothed is important

Bass That wind really bothered the men

Blair There is a tremendous difference between whether you are completely bare or have something on

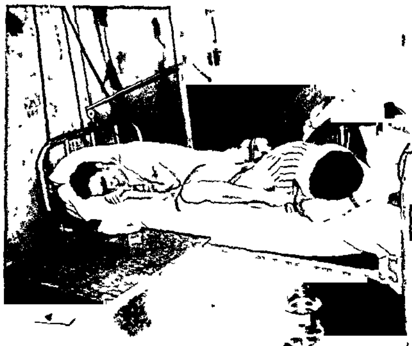


FIGURE 105 Informal (illegal) attempt to rewarm during cold period

Bass In the second study, where we had better control of the wind and it was less than 1 mile per hour, the men wore just shorts

Horvath The sheet and the excellent mattress, which must be at least two inches, adds protection

Bass The men were reclining The point we were after was to get, shall we say, a "pure" cold preparation

Figure 106 shows the caloric intakes of these men and their weights The men were served "C" rations, and they were offered, for each meal, an entire menu which had been analyzed Incidentally, all the rations served came from the same lot, and we analyzed representative cases, by menu and by component All rejects were stored and analyzed also So we knew as precisely as possible, under these conditions, what they were taking in For the first 2 weeks of the control period, it was difficult to get the men to maintain body weight However, by almost pleading with them, we got them to eat even though they did not like the "C" rations We felt this was perfectly valid You can see, in a sense, we succeeded in maintaining

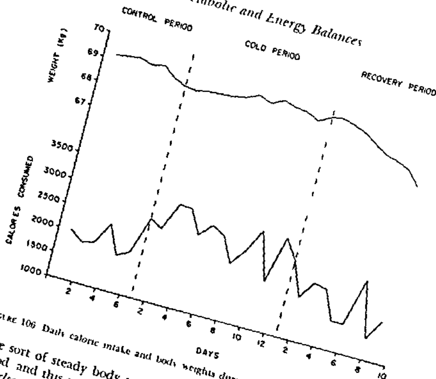


FIGURE 106 Daily caloric intake and body weights during continuous cold exposure

some sort of steady body weight in the control period in the cold period and this is a common observation I am sure

Carlson Is the line drawn right for the cold period if they go in at 6 there?

Bass The first vertical dotted line

Carlson I wondered if the food consumption increased markedly before they went into the cold

Bass The first vertical dotted line represents in terms of weight the morning weight before they went into the cold and the calorie line represents the calories for that day which was a cold day As is evident they maintained weight very well in the cold

They increased caloric intake approximately 400 to 500 calories One of the most interesting things we observed was that although the men did not like the C rations they very quickly displayed an increased appetite which began with their first day in the cold We had no problem in getting them to eat during the entire cold period

Blair In the recovery period I notice the weight came down very markedly Is that water?

Bass No In the recovery period the men, again, lost their appetites, and nothing we could do would make them eat adequately They were in caloric deficit They knew they only had 2 weeks to go

Blair Actually, caloric intake is almost identical here and in the control period It may even average a little higher That was the point that made me wonder if it was water

Bass It wasn't water

Fremont Smith They lost their motivation to eat I want to keep emphasizing "motivation"

Bass They lost their appetite and motivation

Fremont Smith The experiment was over, and there was nothing more to be gained

Bass They had nothing to gain right at the beginning

Blair. Dr Fremont Smith, the data don't show quite that On the graph they are still eating at as high a rate or higher The initial period is 2000 calories or less, and during the recovery period there are several peaks higher and several lower, averaging about the same But they are markedly losing weight That is what I couldn't understand

Bass The water balance did not change appreciably in the recovery period However, in the end there were negative nitrogen, potassium, and other balances

Fremont Smith That will average out a little lower caloric intake

Davis Food intake, when they went into the cold immediately went up, which is different from the situation found in animals

Keller It isn t different from the dog That is exactly the way the dog responds

Davis It is a matter of temperature?

Bass It is a matter of increased appetite, for some reason Throughout the course of my presentation I make no attempt to relate these findings to animals My sole purpose here is to show what happened to our men

Davis I am just pointing out the difference not between animals but between your data and data from rats

Henschel During the recovery period, the men lost about 275 calories a day

Burton I am interested in that sudden weight drop that appears near the first vertical dotted line, when they went into the cold I would like to find out whether that really represents a drop, or whether the draftsman drawing it joined up the points in that way Because, in a similar experiment on far fewer subjects, and not in

such violent cold. Bazett and I (4) found this big drop of weight in the first day. This was associated almost entirely with a fluid change due to diuresis.

But did that drop in the top curve really occur before they went into the cold?

Bass: Yes, that point at the dotted line is the morning weight before going into the cold for the first time.

Carlson: Would you give us the range between the subjects?

Bass: This is the average of five.

Carlson: How much difference?

Bass: In terms of what?

Carlson: In terms of caloric intake.

Bass: From 1900 to 2200 in the control period.

Carlson: Would this correspond with the differences in weight?

Bass: We estimated what the caloric requirements should be, and this fits it quite well. In a subsequent study, we obtained better data on calories.

Travell: Did all the five subjects show the same qualitative changes?

Bass: Yes. Whenever they don't, or whenever there appears to be a change and they don't show it, I will point that out.

Davis: Did using C rations cause the extra variable?

Bass: It caused a variable but also made the logistics of doing balance study much simpler.

We had just finished a very similar study in the heat and had good luck with it, even though we had to coax the men to eat. Furthermore, the logistics of knowing how much and what the men were taking was a big problem.

In our second study we did not use C rations but rather a milk drink of which we knew the caloric density. All we had to do was weigh it out and give it to them.

Reynolds: Was this food heated?

Bass: Yes.

Reynolds: Do you think the fact they were cold and the food was hot increased the appetite for it?

Bass: It might have. We didn't have our psychologist ready to work on this.

Table XI shows the balances. Here again we have averages. For convenience I have broken this down into 4 day periods. It is evident that there is very little change in the balances. During the first 4 days there was a negative water balance. Most of this was the result

TABLE XI

*Metabolic Balances During Continuous Cold Exposure**

Balance Period	Water (ml)	Na (MEq)	K (mEq)	N (gm)	P (gm)	Weight Change (kg)
Days 1 to 4	−278	−25	19	−0.3	−0.013	0.10
Days 5 to 8	353	−56	−13	−2.7	0.314	0.36
Days 9 to 12	−281	29	9	2.6	0.193	−0.18
Cumulative 12 days	−206	−52	15	−0.4	0.461	0.28

* Averages of five men

of the oft repeated observation of negative balances during the first day in the cold

The over all picture especially in the cumulative 12 day balance shows that the men were in remarkably good balance

Burton Will you explain the first column water? Is that water intake?

Bass That is water balance intake minus output

Burton Was it very different from the weight change?

Bass Yes We were not able to find any relationship between weight changes and any of these balances

Keller In the weights shown previously were they corrected for the water negative water balance?

Bass No those were the raw nude weights We had them get up allow them to void and then weigh them

Keller If you corrected for water balance

Bass We don t think the negative water balance is a meaningful water balance except for the transitory negative balance on the first day

Burton Calculated on the water intake?

Bass Calculated on the water intake water of oxidation insensible water loss and water in the food and stools which we analyzed

Burton I did this myself in experiments with Bazett years ago (1)

I satisfied myself that one has to make assumptions about certain factors being in balance in the 24 hour period If you make the same assumptions then the weight change is equally as good as fluid balance

In my opinion this elaborate calculation of fluid exchange wasn t worth while because the weight change would have the same validity as your calculation

Bass Except for this when we began this study we didn t know what we were going to find in terms of tissue breakdown We might have gotten algebraic canceling out of factors in water balance which would not tell us anything about what had occurred in the body We were trying to be as complete as possible and although it involved a lot of pedestrian work we felt it was worth while since probably we wouldn t get another opportunity

Burton I quite appreciate your wanting to do it In using this calculation if the assumptions under which you are making it are examined carefully if it is to mean anything at all one will realize that those same assumptions would mean that the weight change represented the fluid change Therefore I doubt whether making this calculation really adds anything at all to our real information I feel that body water should be measured even though it is difficult

Bass We did that I shall come to that in a moment The picture to this point is one of no physiologically meaningful change in balances of these substances in the cold In other words the body did nicely higher intake and higher output but well maintained balances

Figure 107 shows our attempt to find out what the black box was doing in the face of no change in balance We measured plasma volume thiocyanate space and antipyrine space We found very little change in our four subjects The small increase in plasma volume noted at the fourth day in the cold would not be regarded as meaningful since two men went up and two went down That was the pattern of all the body fluid measurements—two went up and two went down slightly

Henschel That is within the reliability of the technique

Bass Yes So along with no change in water balance as we calculated it there was no change in body fluid compartments

Burton In spite of the fact the calculation of input and output indicates there was a large change in water?

Bass The data on Table X do not indicate a large change It is well within the experimental error of the methods

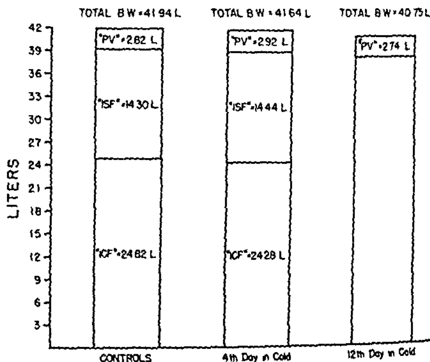


FIGURE 107 Body fluid compartments during continuous cold exposure (58°F averages of four subjects)

Keller Except for the first day when water diuresis occurred but there was recovery from this

Bass Another balance is acid base balance (Table XII) This has not been very extensively studied in man under these conditions although Dr Horvath may know of some work on it Here the three controls show quite a normal picture with regard to the various constituents When they went into the cold on the morning of the first day we obtained another control Then we put them into the cold where they reclined for 4 hours as I have described At the end of those 4 hours we took another blood sample As is evident there is an alkalosis

Horvath Is this arterial pH or venous?

Bass Venous

Horvath With or without stasis?

Bass Without stasis We could not arterialize the blood We have since repeated this in which we arterialized the blood and found no effect

TABLE XII

Blood Acid Base and Electrolytes During Continuous Cold Exposure

Period	Blood pH	Diss CO_2	Serum (mEq/L) HCO_3	Na+	Cl−	K+
3 controls	7.37	1.5	27.5	143.0	101.5	4.31
Cold 4 hours	7.59	0.9	27.3	—	—	—
Day 2	7.31	1.8	29.4	143.9	99.6	4.78
Day 9	7.31	1.8	29.1	142.6	97.6	4.54
Day 12	7.31	1.8	28.1	142.7	101.9	4.58
Recovery Day 2	7.36	1.6	27.9	142.0	101.1	4.15

* Averages of five men

Fremont Smith: Did you warm the arm in taking the blood?

Bass: No. We measured the pH at 25°C and made the correction.

Fremont Smith: The blood flow through the arm will change.

Bass: Yes.

Fremont Smith: What about minute volume? There is probably overventilation.

Bass: That is what I was coming to. We don't think this is necessarily an acute cold response. This was the first day in the cold. The men were understandably upset or at least apprehensive. We feel that this may well be just hyperventilation, that is, respiratory alkalosis due to hyperventilation.

Fremont Smith: Cold itself gives hyperventilation. I wouldn't say steady state, but change from warm to cold will give hyperventilation.

Davis: Especially if shivering is occurring.

Bass: In this case we don't know to what to attribute the acute respiratory alkalosis. It certainly warrants further study.

We have repeated this in two of our responsible investigators, not apprehensive, and were unable to reproduce this. There we checked arterialized versus ordinary cold venous blood and found no effe

of the arterialization We were not able to reproduce this pattern I have merely shown it as a point of interest

During the remaining cold period, the acid base picture gives the impression that there was a slight acidosis This, however, was not consistent for any given day, in that on no one day, of days 2, 9, and 12 did all the men show an acidosis However, every man on at least one of those days showed a lowered blood pH What this means we don't know

We did not notice any change in serum sodium, whereas chlorides decreased There was a general tendency on the part of the men at one time or another during the cold to have decreased serum chloride, but there was no consistent trend The potassium increase on day 2 was shown by every man But, again, we did not find that pattern reproducible on all the men on the other days

We interpret this to mean that in terms of the acid base picture in the blood, there was not a marked change from the controls There is a suggestion of acidosis

Table XIII shows the urinary pattern We collected urines on a 24 hour basis *This is something which, again, may not be statistically* provable but, if it ever attains the dignity of statistical significance

TABLE XIII

Renal Acid Base Regulation During Continuous Cold Exposure*

Period	Urinary pH	Titratable Acidity (mEq/24 hr)	Ammonia (mM/24 hr)
6 controls	6.20	17.8	29.6
Cold			
Days 1 to 4	6.43	18.3	40.1
Days 5 to 8	6.62	13.4	37.4
Days 9 to 12	6.59	12.6	35.4
Recovery			
Days 1 to 4	6.18	13.5	33.8
Days 5 to 8	6.45	15.1	31.4

* Averages of five men

might lead to some interesting conclusions regarding renal function in the cold

As far as urinary pH is concerned, and as far as titratable acidity is concerned, it appears as though the kidneys are on the defensive against an alkalosis yet, if anything there may have been a slight acidosis However, there is an appreciable increase in ammonia excretion So, the total titratable acidity, plus ammonia is actually greater in the cold On the first 4 days, this was shown by all men What we have, therefore, is a picture of the titratable activity paradoxically not responding to acid base defense requirement whereas the ammonia does This is contrary to the usual renal defense mechanism where titratable acidity is the more readily evoked mechanism

Horvath Why does this titratable acid stay down during recovery?

Bass I can't answer that However, it may be due to the reduced food intake But I point this out as a good possibility for future study

In this study, we were fortunate to have, as a collaborator, Dr Sidney Ingbar, who was in charge of the thyroid function studies I shall merely summarize the results of this aspect, since Dr Ingbar is preparing the detailed paper for publication Briefly, using I^{131} we found during the cold period a more rapid loss of thyroidal I^{131} and *an accelerated appearance of organic I^{131} in plasma* These results indicated that a more rapid release of glandular hormone took place in the cold period

Fremont Smith How soon was the pickup?

Bass I was going to come to that By how soon was the pickup ' do you mean when did we find the change?

Fremont Smith The detectable increase in deposit of radioactive iodine How soon after exposure to cold did you detect the change in thyroid function?

Bass We could not *determine* it precisely because of the limitations of the number of injections that could be made

Fremont Smith Is it 3 days or 2 hours?

Bass We don t know Since we were originally limited to three injections of I^{131}, we had planned one injection in each period This posed the question of when in the cold and recovery periods, would be the most profitable time for thyroid measurements It was arbitrarily decided to do this on the morning of the 4th day in the cold and on the 4th recovery day The thyroidal iodine accumulation on the 4th cold day was not significantly different from controls It appeared therefore, that the thyroid results were negative However in order to be more positive of these negative findings we decided

to proceed with the recovery measurement There was a twofold increase in glandular accumulation of iodine on the 4th recovery day Obviously, another measurement was indicated, and we were fortunate to obtain permission for one more injection of I^{131}. This was done on the 14th day of recovery, and it revealed that thyroidal accumulation had returned to control values

The general trend throughout the cold period was a decrease in protein bound iodine, in the face of an increase in thyroid hormone manufacture and release This suggested to us that it might be of value to study peripheral utilization, which we did in the next study I shall describe

Montgomery This is the mean of all subjects?

Bass Yes, this is the mean of all subjects

Burton What is the iodine intake on the C ration? I think it is important in these studies to know what the level of exchange of the body with environment of iodine might be

Bass That is true

Burton Did you not measure intake of iodine in this situation?

Bass *Unfortunately, I don't have the figures on that This,* by itself, is suggestive, certainly In connection with what I will report on the second study, I think we have a picture which is more than suggestive

Behnke The subjects were on C rations during the control period?

Bass Yes and they increased their intake by 20 to 30 per cent

The picture presented by this first study was, at the risk of seeming repetitious, increase in intake and increase in output with little change in the metabolic balances But one of the most important balances in any situation of continued cold stress is energy balance We only had one half of that, calories taken in If you want to consider thyroid function as a balance situation, we also only had one end of that axis, the thyroid gland

To make up for these deficiencies we performed another study in which we concentrated on caloric balances, that is food intake and energy output, and peripheral utilization of thyroid hormone And, to make it a little more sophisticated in terms of thermal physiology, we decided finally to make a measure of thermal regulation The one we chose was rectal temperature

Figure 108 shows one of our subjects by no means our coldest, in a typical position In this study, the room was maintained at 15°C, with very little wind We gave no T shirts and no socks, but everything else was essentially the same as the first study The men did not



FIGURE 108 Typical posit n d iring sec d cold st ly

eat C rations here but fortified milk drinks supplied to us by Dr Spector at the QM Food and Container Institute

We measured resting oxygen consumption and rectal temperature four times during the day We had reasons of an ancillary nature for doing this These measurements were at eight in the morning 12 noon four in the afternoon and at eight in the evening We did this during the control cold and recovery periods The reason we wanted it this way was that previous work had shown that there is a diurnal increase in resting oxygen consumption and in rectal temperature during the day We wanted to see if in this cold situation this diurnal pattern during the day might be in any way altered

Figure 109 shows us the oxygen consumption The two bottom lines which appear to be practically superimposed are the control and the recovery that is the 2 week period before going into the cold and the 2 week period afterward Apparently there is no residual effect on the pattern of diurnal variation The higher curve shows what we got in the cold This is the mean of all the days measured in the cold We examined the data for any changes which might be interpretable as acclimation and found none We therefore felt justified in lumping these just to show the diurnal pattern Here we find the diurnal pattern of oxygen consumption is increased It looks as though the whole curve has been elevated However there is a slight difference At 0800 the increase is relatively greater It is about 20 per cent over that of the controls whereas it goes down to about 15 and 11 per cent over corresponding controls during the day However I won t go into that at this time

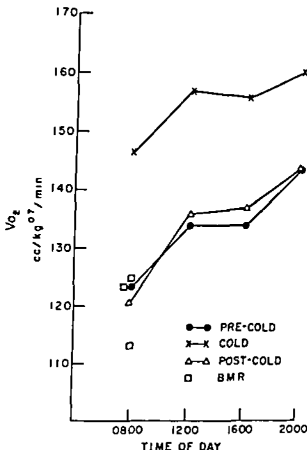


FIGURE 109 Effect of continuous cold exposure on the diurnal pattern of resting oxygen consumption Reprinted by permission from Iampietro P F Bass D E and Buskirk E R Diurnal oxygen consumption and rectal temperature of man during continuous cold exposure *J Appl Physiol* 10 398 (1957)

Keller Did they take the regular meals?

Bass Yes These measurements were made just prior to a meal with the men having lain quietly for three quarters of an hour before the measurements

Keller There is heat production from food

Bass Yes, of course However we had exactly the same conditions throughout the experiment

Keller Couldn t you correct for the heat produced from food? It was only 4 hours after the previous meal

Bass We didn t

Davis When your BMR's were taken, were the subjects on C rations?

Bass The BMR is shown by the squares, there was no change either during the cold or after the cold

Davis Were these also taken?

Bass They were done under rigidly defined conditions

Davis How long was the fasting?

Bass Overnight

Burton These BMR's are just the 'pre-cold' and 'post cold'? The symbols in the graph don't tell us

Bass It is not too clear

Burton You didn't take them during the cold?

Bass We measured them during the cold period and found no changes We took them out of the Cold Room at the end of the first week, and let them rest in a warm room for 2 or 3 hours prior to measurement

Horvath That does not follow the criteria of the BMR They were overnight in the cold room?

Bass Yes, they were Remember, they were sleeping with a blanket They weren't excessively cold I will grant you, Dr Horvath that if we had found a change, particularly an increase, that might have been used as a criticism for interpreting the change as valid

Montgomery Did you say anything about shivering?

Bass We did not study shivering

Montgomery Did they shiver?

Bass Yes On some occasions, however, they were able to lie without perceptible or frank shivering

Montgomery Shivering might have accounted for all the increased requirements

Bass Yes, although we prefer to say shivering or some other form of heat production

Horvath It is interesting that the preparation for taking the metabolism of men who are shivering violently has a tendency to calm them If the degree of shivering is measured crudely there does seem to be a decrease while the metabolic records are being made

Travell Did they seem to shiver less at the end of the 12-day period in the cold?

Bass We attempted to assess this In three or four of the men we didn't pick this up on measurements of oxygen consumption All we got was a very crude subjective evaluation Four of them felt they were doing a little better in the cold They still were not too happy

about it In Figure 109, therefore, there is relatively greater increase in oxygen consumption at eight in the morning

Figure 110 shows the rectal temperatures The symbols speak for themselves As was the case with oxygen consumption we found no change in pattern throughout the course of the cold exposure Only at 0800 is the cold rectal temperature different from that of the control or the recovery which is, incidentally, again superimposed on the control pattern The other three rectal temperatures—at noon four in the afternoon, and eight in the evening—are no different from the controls or recovery

Fremont Smith When did you take the blankets away?

Bass We took the blankets away around seven The men got up voided were weighed, went back to bed, and lay down The elevation at eight in the morning might be an overshoot I think the elevated rectal temperatures and oxygen consumptions are mutually supportive in this regard

Up to this point we have, in terms of caloric balance an increased

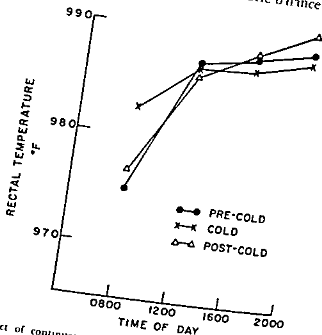


FIGURE 110 Effect of continuous cold exposure on diurnal rectal temperatures Reprinted by permission from Iampietro P F Bass D E and Buskirk E R Diurnal oxygen consumption and rectal temperature of man during continuous cold exposure *J Appl Physiol* 10 398 (1957)

turnover with intake balanced by output Incidentally the increased oxygen consumption was percentagewise approximately equal to the caloric intake In the face of this increased turnover the body is relatively unchanged with regard to the parameter we are most concerned with at least in this case—core temperature

Fremont Smith Had you obtained the rectal temperatures in the morning before you took off the blanket and before they voided you probably would have had a lower rectal temperature

Bass Yes I don t have the slide but we studied the men during sleep That is the subject for a paper about to be submitted for publication (5) Actually they were lower This is characteristic of the sleep pattern

Behnke Are you going to show any other skin temperatures?

Bass No

Behnke Do you have the big toe temperature

Bass We had that during sleep Nocturnal toe temperatures were maintained approximately 15°C higher on later nights in the cold period than on earlier nights

Behnke In a similar type of experiment by Spealman (6) with subjects exposed over a period of 30 days it was brought to my attention that one of the individuals certainly developed cold injury (paresthesia) of the toes

Bass We got something of that sort

Behnke Were they barefoot the whole time?

Bass In this study they were We got chilblain like symptoms

Behnke Wall temperatures were 15 6°C

Bass They wore sandals on the floor

Pace These men had a blanket

Bass At night they had a blanket

Behnke Why didn t you take skin temperatures?

Bass We just didn t

Behnke You don t know how cold these people were?

Bass No Not in terms of the heat content of the body That is true We don t know *how* cold but we know they *were* cold which was what I said we were interested in

Horvath In terms of days their temperature fluctuations are similar to the normal pattern of fluctuation but they do occur at lower level I didn t bring the slides but we have continuous measurements of rectal temperature and about fifteen skin temperatures

It is interesting and I have never quite agreed to this before

especially in Dr Burton's presence, that they do have hunting during the night

Bass We have some data on that It is being published (5)

To go back to the problem of thyroid activity, we injected radiothyroxin, and, from disappearance curves, we were able to assess *peripheral utilization of thyroid hormones* This is the method described by Ingbar and Freinkel (7) Briefly, daily degradation of thyroxin increased in four of the five subjects during cold exposure, and decreased to less than control values during recovery To illustrate in *one of these four subjects, the half time of degradation was* 5 6 days, in the control period, 4 4 days in the cold, and 7 2 days in recovery The one man who showed no change throughout the study was by far the most uncomfortable of the subjects Whereas the other *four men were uncomfortably chilly, this man was apparently under*going an ordeal of cold discomfort Fortunately, he was highly intelligent and well motivated to continue despite this

The results of the second study, therefore, show again, this time *in terms of energy balance, an increased turnover In terms of core* temperature, the body was successfully resistant to hypothermia despite the fact that the men were uncomfortably cold for 16 hours of the day for 12 to 14 days

Pace May I ask, Dr Bass, how your values would compare with [illegible] to get the scale?

Bass By this technique hyperthyroid subjects have half times something like 2 or 3 days, compared to an average of 5 to 6 by this technique

Carlson Would you tell us a little more about the method? Is this *measuring the rate of disappearance from the blood?*

Bass Yes

Carlson Did you make any analysis of feces or urine?

Bass Yes urine was analyzed

Horvath Did you do feces? Most of the thyroxin goes out through the bile That is the major source by which it goes out of the organism

Bass That is a good point In this study we did not collect feces I will say this, the feces were almost nonexistent in that second study, because of the liquid diet However, in this method, a basic assumption is that fecal collection of I^{131} proceeds at the same rate as loss

of radioactive thyroxin from plasma Ingbar and Freinkel have presented data to support this (7)

Carlson Van Middlesworth s (8) work has shown that the amount of material going through the tract clears thyroxin An animal s thyroid requirements can be increased just by having bulk in the diet because of the clearing

Burton The amount of radioactive thyroxin is minimal? So there is no question of its affecting the homeostatic control mechanism of the pituitary There is a very minimal amount of added thyroid to the endogenous thyroid hormone

Bass Yes Incidentally in thinking of thyroid we don t know to what extent physical activity increased thyroid activity It certainly might be expected to increase peripheral utilization We hope to do a study in which we have a similar group which instead of living in the cold will be on a regimen of exercise equivalent to shivering in cold to see if this is a factor

Regarding the thyroid two questions arise First what is the relationship between the increased thyroxin production and increased peripheral utilization? Which comes first On the basis of past animal experiments and on the basis of our data one can speculate that the increased production is a consequence of the increased peripheral utilization Another question is what is the relationship of the alteration of thyroxin degradation to the change in energy turnover? That is is the increased utilization a cause or consequence of the increased energy expenditure? There again we can only suppose that the increased degradation is a result of the increased energy expenditure

Travell I would like to point out that a fall in the serum protein bound iodine during cold is quite compatible with increased peripheral utilization of thyroxin because tri iodothyronine lowers rather than raises the binding capacity of the serum protein for iodine This would be important

Pace Where are the adrenal cortex data?

Bass I don t have a slide showing them but we measured eosinophils and 17 keto-steroids and leukocytes Eosinophils by direct counting method did *not change significantly throughout the entire* experiment Nor were there any significant changes in 17 keto steroid excretion

Behnke Did you change the diet during the course of the experiment?

Bass No except to let the men eat more *ad lib*

Behnke When did they get the milk diet?

Bass In the second study right from the beginning of the control period

Behnke From the beginning of the control period through?

Bass Yes

Behnke Did you add salt? The milk diet is practically a salt free diet

Bass This is a fortified milk I might add we did give them toast and salted butter We do have an estimate of the salt intake It was adequate

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FIELD STUDIES ON COLD ADAPTATION

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WHEN ADAPTATION IS DISCUSSED a different set of circumstances must be considered than when discussing physiological mechanisms alone According to the dictionary definition adaptation as applied in the biological context involves a modification that fits an organism more perfectly for existence under the conditions of its environment adaptation is applied especially to a process of evolutionary change in structure and function

So when we study adaptive physiology we must consider not only the physiological references and the environmental situation but also the fact that the word adaptation carries an evolutionary connotation

We must also keep in mind the fact that the study of environmental adaptation can be suitably carried on only when it is made with reference to the fully developed adaptability of the animals or people under consideration For undeveloped adaptability may be quite obscure unless it is developed by use to its real full and perfect expression

Full development of adaptation requires not only the development of an ability but motivation For example the diving of a seal is a regular exercise in suspended breathing which is carried on continuously and is part of its normal pursuit of food escape from dangers and ordinary travel All seals then are trained to the full extent of their ability for holding their breath Ordinary examples of men could not properly be compared with seals to find out our adaptability to diving

The magnificent Australian mile runners at the present time are showing that man is adaptable to run a mile in 4 minutes Further the most amazing thing about these trained men is that they can

run a mile in plus or minus one second of the predicted time when they are fully trained and motivated to do so

I think that illustrates some of the advantages to be derived from utilizing fully trained and motivated people and animals in experimental work, and probably it is a necessity when adaptation is involved in the study

We have carried on a number of studies of the reactions to cold in Arctic populations, because it seems a safe conclusion that Arctic populations are, by reason of their successful existence in that environment, truly adapted to live in a cold environment

A thermal environment can be very complex I have noticed that many people speak of "environmental" temperature when they know only the temperature of the air To dress up a simple and often quite inadequate measurement of the temperature of the air by the designation of "environmental' or even 'ambient" covers the animal's thermal situation with confusion

The essential way in which we looked at adaptation to cold, as I presented it at the preceding Conference, emphasized the fact that the animal maintained its body temperature with a fair degree of constancy and that modification of internal body temperature, as a form of adjustment to climate, was not found to be of any great importance Likewise, the basal metabolic rate seemed to be an operating characteristic of the metabolic machinery which couldn't carry out much variation successfully and permanently, such as would be likely to suit an animal population or a normal human population to continued existence in the cold (1)

The conspicuously variable and therefore adaptive function in the description of the animal as a heat machine is the factor of insulation At first because it was obvious we spent a considerable amount of time in the examination of fur as the insulation of the northern animals But the fur becomes one of the less interesting parts of the insulation system when you consider that it is invariable within a brief period At least we can t see how it can vary, as the requirements for the escape of heat or retention of heat change so rapidly

So we were interested in finding that the cooling of the extremities was in some animals (2), quite conspicuous and that in bare skinned animals such as swine (3) and the rather thinly fur covered hair seal (4) the entire body surface could vary its temperature and thus indicate the means for conservation or increased release of heat

Although we considered those examples of the insulating system last they nevertheless probably represent the simplest example of

the physiological insulation and are certainly the most easily observed and measured

That, of course, has brought us to the point of how we are going to look at human adaptation to cold One of the obstacles to the determination of true human adaptation to cold is that people don't like cold, and none of us is commonly willing or even in a position to become adapted to cold

We have confined soldiers to cold rooms, and, just as Dr Bass did, we have found some of the acute effects or responses which a man makes when suddenly exposed to cold But soldiers are not motivated to become adapted, nor do they represent what should be considered as a population

It is strange, when you think how much of our physiology is derived from observations made upon ourselves, our colleagues, and upon such other examples as we can order about in the laboratory that none of them is normal in the sense of being normally adjusted to the type of conditions which are important in molding the characteristics of a natural population

We have tried to determine what kind of people might be truly adapted to cold We were convinced that the cold to which they could become adapted by their physiological mechanism alone certainly would not be Arctic cold, but only the sort of moderate cold which appears in a warm temperate situation or even an actually cool tropical one

The methodology of setting up an expedition for finding people who, as a population, were really adapted to cold, was quite complex But with his characteristic energy and skill, Scholander devised methods and aroused the enthusiastic interest of a number of young colleagues and trained them into teams which could operate effectively as task forces in a number of areas I am simply summarizing some of his experiences and results rather than presenting any contribution of my own It would have been better had Dr Hart done this because he has just returned from participation in one of the field experiments

In early studies, Scholander and his Norwegian colleagues showed that the critical temperature for the elevation of metabolism by cold was the same in nude Norwegians and Lapps (5) Lapps occupy a similar climatic position and wear about as excellent, warm Arctic clothing as do Eskimos There is no sign, with regard to their essential basal metabolism or their critical temperature, that Lapps and Eskimos differ from Norwegians

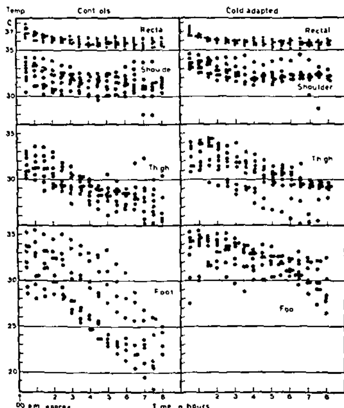
The next project Scholander (6) arranged was to motivate a group of young university students from Oslo for the development of their adaptability to cold by offering them the opportunity to spend about 6 weeks in one of the areas above the timber line where in August and early September, frosty nights and frequent chilly days are the regular regime These young men were to wear only very light clothing and to spend their time in fishing, hunting, hiking biological field studies and whatever active pleasures they enjoyed Since they escaped from the university for this freedom, they were devoted to its pursuit At the end of about 6 weeks, these young men were examined for some of their thermal reactions and metabolic responses

Figure 111 indicates the situation which was found These young men were certainly exposed to cold and they were chilly But their regime had to be consistent with health and good spirits or they would not have provided a practical experiment

Figure 111 gives the records of temperature measurements made at various places over the body, while the men were asleep in a thin sleeping bag at a temperature about 3°C There are several reasons for the utilization of a cover I think Dr Bass indicated some of them One is that civilized man is somehow comforted by covering for the naked condition is not esthetically or physiologically a happy one in which adaptability could be fully developed

The bag provided less than 1 clo of protection and was not nearly sufficient to keep the unacclimated men comfortably warm, as is shown by their recorded temperatures Their body temperature declined a little, as is shown by the rectal temperatures but not much differently from the cold adapted men More decline was shown in the skin temperature on the shoulder, on the thigh and on the feet of the men unaccustomed to exposure There was remarkably good maintenance of the cutaneous temperatures by the men who were accustomed to exposure

Figure 112 shows the metabolic provision of heat by the two groups of men When they were naked at 20°C, which is rather chilly for the ordinary person their metabolic rates scattered quite variously But in general, the men who had been earlier exposed to cold had appreciably higher metabolic rates and that was somewhat more consistently demonstrated by what was going on in their attempts to sleep in the sleeping bag at 3°C It turned out, in summarizing these data that the metabolic rate of the men who had been exposed to cold was significantly greater while asleep than was that



...RE III Rectal skin temperatures in test runs where the men rested naked in sleep bags during the night at around 3°C. Reprinted by permission from Scholander Hammel H T Andersen K L and Lovning Y Metabolic acclimation to cold an J Appl Physiol 12 1 (1958)

he men fresh from the city That seems to be the reason for their lity to sustain the temperature of their skin at levels which were too uncomfortable which didn t obviate shivering but which ertheless permitted the acclimated men to obtain adequate rest ing the cold nights Although as they thought about it later y recalled that they sometimes did feel chilly

Iart I am going to object here I am not convinced that there is evidence of a difference in metabolic rate between the acclimated l control subjects when the variability is taken into account

rning You have a point there all right I am repeating Scho-

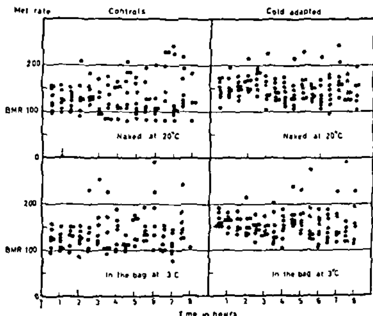


FIGURE 112 Metabolic rates in all night test runs where the men rested naked at 20°C and in bags at 3°C All data are given in the percentage of the pre-exposure BMR Reprinted by permission from Scholander P F Hammel H T, Andersen K L and Loyning Y Metabolic acclimation to cold in man *J Appl Physiol* 12 *1 (1958)*

lander's conclusions and inclined to agree with them for reasons other than are now apparent

Burton I emphasize the fact that a presentation of mass data in such a situation must be by paired statistics " The same man should be compared before and after The scatter will not give the answer However, it might not apply if one used individual groups, with paired statistics instead of 'group statistics'

Irving I think that would be a superior demonstration

Pace I think also it may be important that, in the cold adapted group the variance seems to be much less than in the nonadapted group

Irving I would be prejudiced to search for that consistency after training but I am not convinced that statistical treatment could either confirm or deny my opinions

Davis These are the same individuals, are they?

Irving No they are not

Burton Then of course, my point doesn t apply

Irving They were not the same individuals examined before and after acclimation They were simultaneously examined groups of men who had been accustomed or unaccustomed to exposure to cold

The group on the left hand side of Figure 112 are simply untrained men and perhaps part of their variability comes about from that because we can usually recognize unpredictability in the performance of untrained men or animals

Horvath Don t some of those controls later become the cold adapted men?

Irving They didn t stick it out long enough They were only there for a short time since they were busy about other occupations Figure 112 also shows the period through the night beginning at eleven and extending to about eight in the morning These are measurements of oxygen consumption or metabolic rate from oxygen CO_2 measurements

Burton Every hour?

Irving No Sampling was more frequent The men slept with their heads in a sort of plastic covered box through which air was continuously ventilated into measuring spirometers and at about half hour intervals samples from the spirometer were removed and analyzed for oxygen and carbon dioxide

I think Dr Hart s and Dr Burton s comments are pertinent here In Figure 111 the difference in the foot temperature of the two sets of men was quite inescapable and the difference reported in the ability to sleep which early became established as these men continued their experimental exposure to cold and the control subjects was very marked Because although the control subjects thrashed around and shivered and shook and put out nearly as much heat as did the adapted people they nevertheless were completely miserable and quite unable to obtain rest

Carlson I think we should point out the fact that the temperature data as well as these data confirm several other studies such as Balke (7) did many years ago This is a common observation on groups who live in the cold

Irving Of their developing adaptation?

Carlson Yes

Irving There are many convincing examples of that kind personal experiences of developing comfort under exposure if it is not too severe and if it is within the range to which the individuals can become adjusted

Fremont-Smith There was difference also if the unadapted group did not sleep and the others did You are measuring one group awake and uncomfortable and the other asleep and comfortable

Irving The remarks that people make about how they didn t get a wink of sleep the night before are often found to be only about 5 per cent correct

Davis Was there a shivering difference between the controls and the cold adapted?

Irving There was less shivering in the cold adapted but they did shiver nevertheless That was their opinion at the times when they awoke and it was confirmed by examining these same individuals with the electromyograph and electroencephalograph immediately on their return to Oslo The electrical records showed their frequent *shivering while they were sleeping in a cool room*

Davis They were of course relatively warm when they slept at 3°C in the bag weren t they?

Irving The cold adapted people maintained a fair temperature but it was still sufficiently cool to induce some shivering

Carlson It was a 1-clo bag

Irving They shivered and slept the controls shivered and didn t sleep

Davis You don t know the relative quantity of shivering You say they shivered and didn t shiver

Irving I wouldn t know how to estimate it in the field

Pace The foot temperature dropped in Figure 111

Davis Yes

Irving It shows the inability to sustain comfortable temperature

Davis *Heat loss from the adapted men is greater by virtue of the* higher extremity temperatures than the unadapted

Irving The means of this sort of adjustment to cold are apparent It is indicated that it is accomplished by an increasing ability of the metabolic machinery to produce heat while still permitting the other functions of the resting to continue

There was no reason to think that this change which was brought *out in the matter of a few months* represented other than partial development of the adaptability latent in people and it might very well not show the full limit or even the means by which man could be best adjusted to a cold condition

So Scholander figured on going to a place where a people had been accustomed not only for their lifetime but for generations in the past to existence in the naked condition And that was to be

found in the interior of Australia, among the aboriginal people there who had also been examined some years ago by Sir Stanton Hicks and his colleagues (8) But there were some questions about the meaning of the results of the studies of Hicks and his colleagues, which it was evident could now be better resolved by the methods which Scholander and his associates had prepared

Figure 113 shows the sleeping habits of the native people who in that particular group in Central Australia were quite accustomed to sleeping entirely nude at night Dr Hammel is shown going through the maneuvers of application of the thermocouples to the skin surfaces This shows the cover for the head through which air was circulated, and it also shows the natives custom of constructing a small brush shelter and how they often put a little fire by each man Sometimes two men sleep side by side between two fires Thus while they sleep naked they obtain both shelter and warmth by their normal sleeping habits



FIGURE 113 Native lying between fires His head is being fitted into respiratory hood (note thermocouples) Right hand side is an empty hood showing ample rubber sleeve for neck fit Reprinted by permission from Scholander P F Hammel H T Hart J S LeMessurier D H and Steen J Cold adaptation in Australian aborigines *J Appl Physiol* 13 *211 (1958)*

Figure 114 shows the results of some comparisons of the metabolism and temperature in the white group who served as the control or perhaps, the bad example, and the native who was the regular and, as it turned out, adapted person There wasn t any marked difference between the oxygen consumption of the white and the native under these conditions

The temperatures were somewhat similar on the hip, not at all different as regards the rectal The temperatures of the foot facing the sky and of the hip facing the sky were quite different from the temperatures of the skin which faced the fire With the great variety of environmental conditions for heat loss, the fire heating on one side, and radiation loss to a cold sky on the side away from the fire, it was quite impossible to measure the effect of cold

How the two sets of individuals met the situation is best demonstrated by the fact that four times during the night the Australian awakened and replenished the fire while the white men woke up ten

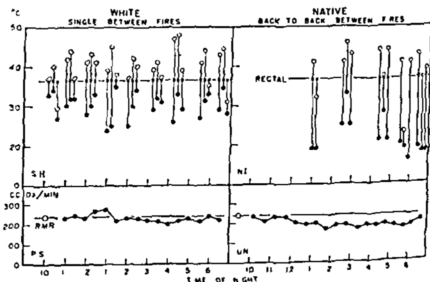


FIGURE 114 Skin temperatures and metabolic rates in two whites and two natives sleeping *proper bush i.e. naked between fires Upper panel surface temperatures* facing fires open circles facing sky filled circles rectal temperatures X Vertical bars arranged in groups of three represent from left to right measurements from chest hip and foot taken at the same time Lower panel O_2 consumption Air temperature dropped from +5° to 0°C during the night Radiation temperature of sky registered −20°C Reprinted by permission from Scholander P F Hammel H T Hart J S LeMessurier D H and Steen J Cold adaptation in Australian aborigines *J Appl Physiol* **13** *211 (1958)*

or fifteen times in fact they said they couldn t sleep at all This illustrates the economy in behavior of the two groups Not only was the white man less accustomed to cold but he was unskilled in the maintenance of his fire and was morally and mentally unprepared for the whole performance

Fremont Smith Was the heat of the fire applied mostly to the lower extremities?

Irving The native is apparently able to rotate himself periodically in order to make more efficient use of the heat available from the fire So there is a combination of habit and probably some physiology here but the physiological adjustments could not be clearly demon strated However Figure 115 indicates how some of that was ac complished

Bass Is the body size of the whites comparable to that of the native?

Irving They are not incomparable but the natives are usually more slender and a little lighter As I recall the figures the difference in the average was not more than a few kilograms

Hart Whites were 60 70 71 and 85 kg natives were 53 54 55 58 59 and 67 kg

Bass I was thinking of the oxygen consumption

Hart Per unit body size the resting metabolic rate of the native was slightly greater than that of the white These results are for two individuals not averages of many

Montgomery When the white man got up he also had a fire and he also did something about the fire Where is his foot temperature?

Irving These are a few illustrative measurements made in feeling out the tactical situation in order to develop what kind of experi ment would give significant physiological results

I think this is sufficient to indicate that significant or clear physi ological evidence could not be derived under these conditions So they went to the other situation in which the people simply slept naked without fires Then at least the thermal gradients could be fairly well defined

Figure 115 shows the results of the skin temperature measure ments in some of these cases The top set of figures shows how rectal temperature declined in both sets of subjects The chest tempera tures showed no clear difference

Gradually down toward the feet the temperature on the surface of the native subjects diminished It is difficult however to make that conclusion even from quite a large number of spot determi

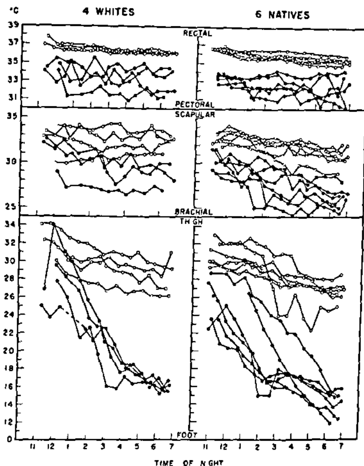


FIGURE 115 Rectal and skin temperatures of whites and natives during same bag tests as in Figure 116 Reprinted by permission from Scholander P F Hammel H T Hart J S LeMessurier D H and Steen J Cold adaptation in Australian aborigines *J Appl Physiol* 13 *211 (1958)*

nations, because any means of summation applies arbitrary weighting to the significance of one area as against another

By the most careful method of summation which could be applied, as will be shown later, it was indicated that the periphery of the native subjects cooled considerably more than did the periphery of the white subjects

Figure 116 shows the metabolic records for the two groups of people Again, the regularity of the native with regard to his metabolic rate and the marked irregularity of the white man are evident

These records mean in the one case, that the native was sleeping peacefully and relaxed The evidence for relaxation was reported to be the audible record of snoring, which is not now presentable

Siple Is it safe to assume that the white man was not accustomed to lying on the same hard surfaces and conditions of that nature?

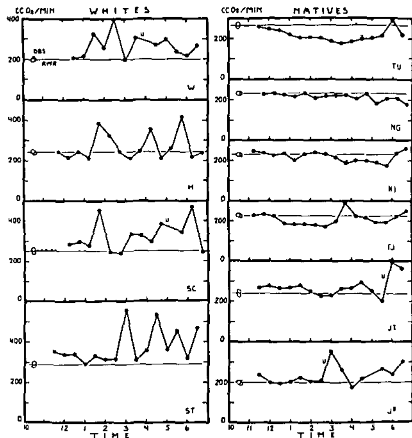


FIGURE 116 All night O_2 consumption in whites and natives resting in sleeping bags at air temperatures decreasing from average of 5° to 7°C in the late evening to 2° to 0°C at dawn

RMR = resting metabolic rate

DBS = DuBois standard

U = Urination

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Irving That is probably a factor Altogether the white man was miserable

Siple It may have been in addition to the cold the hardness of the ground and things of that nature that affected his sleeping heat production loss etc

Irving Yes Dr Hart can probably tell you that

Hart The white man was in the sleeping bag

Siple Was the native in the sleeping bag too?

Irving Yes again in a thin bag You will see in spite of or probably because of his threshing about there was more than just shivering activity on the part of the white man but in spite of his elevated metabolism he still could not sustain the temperature of his extremities to a comfort level

Horvath It is amazing that these whites were so much more irregular they were so in Australia and Norway where the temperatures were of somewhat the same order of magnitude Therefore the response must be more closely related to the type of situation

Hart I do not think that the whites in Australia were more variable than those in Norway

Irving Dr Hart you have called our attention to the irregularity or the scatter of the points representing the metabolism of the Norwegians They were not connected to represent the individual variations as these are Each line represents the record of one individual and shows the periods of his metabolic fluctuations

Siple What was the bag lying on?

Hart The bag was on the ground outside

Siple Solid ground?

Hart That is correct

Siple I stress this because unless the white man had slept on the ground or was accustomed to sleeping on the ground the hardness and the pain of pressure didn t correspond to normal sleeping conditions and so may have contributed a great deal to his uneasiness while going to sleep

Hart Hard ground was a contributing factor but not the main discomfort

Burton This does not apply to Dr Scholander and Dr Hart They are accustomed to lying in sleeping bags on hard ground

Carlson Are the temperatures we saw temperatures in sleeping bags or were they taken while the men were lying out by the fire?

Hart These were taken in sleeping bags

Carlson In Figure 116 were the men in sleeping bags?

Hart That is correct

Lyman Did they get up and poke the fire?

Hart There was no fire

Behnke The natives were stretched out in the bag and on the ground, too Were the white men curled up in the sleeping bags?

Irving What was the actual situation there Dr Hart?

Hart The sleeping bags were tapered so that it was very difficult to do much curling up

Irving An effort was made to forestall curling up by shaping the bag so that the occupant couldn t move around much

Davis This might be a parameter of adaptation, mightn t it?

Irving It is certainly a behavioral factor Figure 117 presents a summary of the contrast of the two types of adjustment in the response of the unadapted white man to cold exposure Increase in metabolic rate, but inability to maintain the temperature particularly in the feet, are evident The acclimated Norwegians showed, on the whole, a greater elevation of nocturnal metabolism and retained for some little time an increased resting metabolic rate By that increased expenditure of metabolic energy the acclimated Norwegians

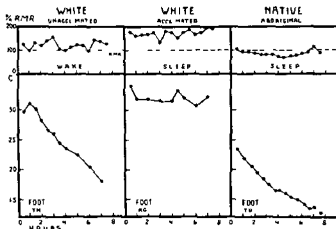


FIGURE 117 Examples of two known modes of general acclimation to cold in man as evidenced by all night bag tests

Left = unacclimated white control

Middle = metabolic acclimation of whites in Norwegian mountain experiments

Right = *insulative cooling in Australian aborigines*

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maintained the temperature of their feet at a comfortable level, so that they could sleep quite well

The metabolic rate in the aborigine not only didn't increase during the cold sleeping conditions, but it actually declined, as one might rather normally anticipate in a condition of true sleep However, he allowed his feet to cool and didn't seem to mind

Fremont Smith He had better insulating qualities

Irving He utilized tissue cooling as part of his means for a heat economy which made unnecessary the expenditure of more metabolic energy Of course, tolerance of such a degree of cooling by the foot is remarkable to observe in a human subject

Horvath You would have to say the white man never becomes acclimated, and that there is no such thing as an acclimated white man If the native is built with the same structure and formation as any other human organism, he is, therefore, an example of true adaptation to the environment He has the ideal adaptation Therefore, it can never be said that a white man is so labeled as being acclimated

Fremont-Smith Just partially acclimated

Irving I am not sure that the term *acclimation* requires the development of that faculty to its utmost I think it would be foolish to set any limit on acclimation because you would have to expose a man for an entire lifetime before you knew what the limit of his adaptability was Then you would begin to run into other difficulties For example, is this the development of innate or acquired adaptability? I don't think it worth while to pursue these distinctions in definitions much further

Travell Isn t there a difference in the thickness of the skin of the foot in the white man and the native? Is there, perhaps, a difference in the depth of the sensory receptors below the skin surface in these two groups?

Irving I don't know The skin of such people appears to be just like our skin, doesn't it, Dr Hart?

Hart It doesn t appear to be different

Talbott The native doesn t wear sandals or shoes?

Irving No The bottom of the feet might be more calloused by walking without shoes, but the white men might have had corns and calluses

Fremont Smith I think Dr Travell made a real point We shouldn't dismiss it because we don't know We should bring it out and emphasize and study it

Irving I am glad she reminded us I was going to remark that it indicates that one factor in their adaptation is the fact that the sensory receptors on the skin surfaces or near the surfaces are pitched at a lower level for excitability It is possible of course that there is some central inhibition But I think the situation is presented where one component of the peripheral adaptability can very well be examined

Behnke Was the temperature measured on the dorsum of the foot?

Irving Dr Hart can answer that

Hart These were on the dorsum

Behnke There wasn t very much difference in thigh temperature between the native and the white man

Irving No there wasn t much difference The Norwegians during August and September slept with thin covering through nights and often days which were frequently frosty or near freezing

Fremont Smith The situation is different in Australia where the days are very hot

Hart The days were not very hot at that time of the year The air temperatures occasionally attained 65°F or higher

Fremont Smith Was there a great deal of sun radiation?

Hart Yes the skies were generally clear

Pace With regard to native populations and their tolerance of low foot temperatures the Sherpas in the Himalayas frequently wander around on the snow completely barefooted I think it is one of the most remarkable sights I have ever seen While they do have extensive cornification of the sole the dorsum of the foot still appears quite normal How these people can tolerate what is obviously a temperature near freezing without getting frostbite let alone apparently suffering no obvious discomfort is a minor mystery

Irving It is a mystery only because we haven t examined it Some of the factors involved are perfectly obvious We have already made suggestions as to how they can be examined (9) Adaptation of nerve and softening of fats in cold tissues suggest one of the clues in starting the analysis of the means of tissue adaptation

Siple The men we considered as being the best adapted or enjoyed most going into the cold that is the ones who could stay out wearing the least amount of clothing and not mind it such as our dog drivers had one characteristic in common They generally had cold feet when they went to bed at night and also didn t get to sleep very easily until their stored heat was paid back I have found in my

own case that I can t get to sleep until my feet warm up It also seemed to be a characteristic of many of our men They could tolerate low skin temperature all the time they were up and around, and they were obviously in a vasoconstrictive state Their foot temperatures might go quite low However, when they tried to relax to go to sleep they had difficulty until the feet warmed up This was a complaint expressed time after time by some of our trail men Whereas the men we considered the poorest acclimated, did not seem to have cold feet when they went to bed and they apparently could go to sleep more easily

Figure 118 gives H T Hammel s summary of heat conduction from the skin Although the summation of heat conducted from temperature measurements requires weighting and factors which

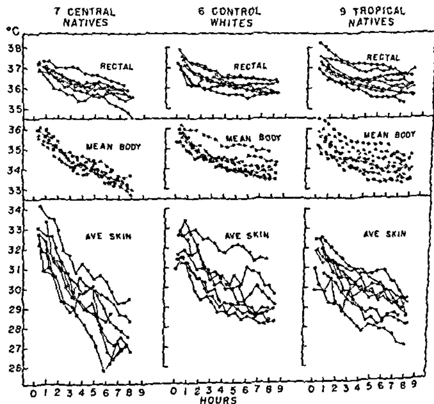


FIGURE 118 The average thermal and metabolic responses of six control whites and seven Central American natives in summer Reprinted by permission from Hammel H T et al Thermal and metabolic responses of the Australian aborigine exposed to moderate cold in summer *J Appl Physiol* 14 605 615 (1959)

cause me to be dubious about its precision, I think that the summation gives a fair representation of the difference in thermal and metabolic responses of the white and the native

The rectal temperature of the native commonly fell a little lower than that of the white The estimated temperature of the body as a whole, taking these arbitrary assignments of the temperature as indicated from surface spots and rectal temperature, indicated that the native allowed for greater cooling of his tissues than did the white

So, by assigning to spots indications about the full skin area, the skin of the native cooled to a marked degree more than did that of the white The metabolism of the white man was high and it was erratic, whereas that of the native was rather steady and considerably lower

If, then one makes from the whole situation as was significantly accomplished by H T Hammel the conduction of heat from the surface, it turns out to be considerably higher from the white than it does from the native Altogether these indications are quite consistent with each other and with the behavior of the people, and they seem to indicate the nature of the adaptation of the native

Figure 119 shows problems of adaptation It was desirable to see if the adaptation of these Central Australian natives remained the same in the summertime, when the temperatures were during the day at least considerably warmer or hot So H T Hammel went again with an expedition to examine the natives from Central Australia in the summer and, in substance obtained results comparable with the winter records showing no sign that the adaptation that appeared in winter was diminished by summer heat However his results also brought out the fact that the summer nights are by no means without some cool stress So it may be possible that this does not conclusively demonstrate that it is a racial rather than acquired adjustment to cold

Fremont Smith Racial and acquired because the men have been living in this environment for many generations

Irving That very important issue seems almost within our grasp and yet we haven t quite been able to seize upon it However, to go further into this area they went to the tropical part of Australia, in the vicinity of Darwin and examined some of the native people whose lifetime and presumably that of their ancestors had been spent in a truly tropical climate

The results shown in Figure 119 will indicate that the white and tropical natives, when examined in the summertime, did not show

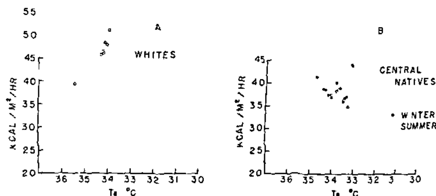


FIGURE 119 The average thermal and metabolic responses of four control whites and six Central American natives in winter Each variable for the natives is significantly different from the same variable for the control whites at the 01 level for each period from the 3rd hour to the end Reprinted by permission from Hammel H T *et al* Thermal and metabolic responses of the Australian aborigine exposed to moderate cold in summer *J Appl Physiol* 14 *605 615 (1958)*

what are probably significant differences in their responses while sleeping cold and that the tropical native while better than the white was not so competent not quite so economical as the native of the cooler central regions

This again is not conclusive that the difference is a racial trait because as the investigators remarked natives living in the vicinity of Darwin are quite sophisticated and commonly wear clothes So by habit alone they may have lost some of the faculty of maintaining comfort and physiological economy while sleeping in a cold period

These experiments correlate in an interesting and consistent fashion They indicate methods of observation which lead to significant studies of acclimation and possibly toward physiological distinctions among various groups of the human species We may be looking at true adaptation as compared with what we commonly call acclimation

Since a number of people have already participated in these expeditionary methods of experimentation and they are enthusiastic about that sort of operation further tests of this sort will be continued in other situations We can fortunately leave the ultimate decision as to whether this is true adaptation in the biological sense to empirical studies which will be carried on during the next few years

Bass How would you classify the Eskimos relative to these abo-

rigines as an acclimated group? Would you put them in the same category?

Irving We haven t satisfactory evidence yet That is something for Dr Hart They are his Eskimos The Alaskan Eskimos are a little hard to manage

Hart There is a rather striking racial comparison recently re ported from the Arctic Aeromedical Laboratory where the white subjects were compared with Negroes and Eskimos during the sum mer This study as I recall tends to show the Eskimos compensate by greater heating of the periphery during exposure to cold (10)

Bass That is greater difference?

Hart That would contrast with the adaptation in the aborigine

Barquist This adaptation of the Australian would be inappropri ate if he were in danger of frostbite It seems to me the adaptation of the Norwegian would protect his extremity from frostbite at the expense of a metabolic rate increase whereas the Australian would allow himself to cool

Fremont Smith Maybe he doesn t frostbite at the same tempera ture especially if there is a difference in nervous sensitivity

Irving There is a difference in the reaction to cold of superficial tissues after they are adapted to cold

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